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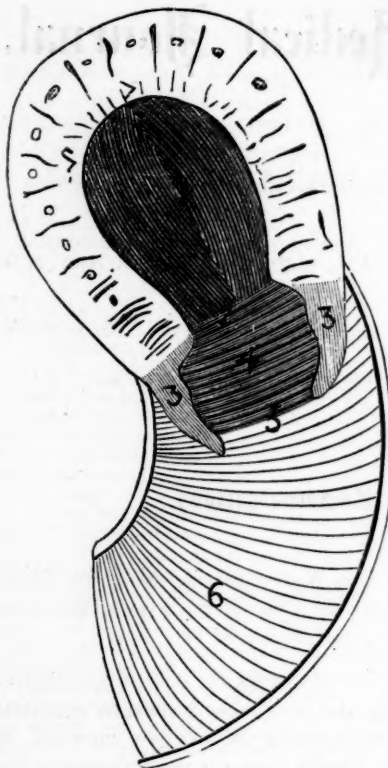
ARTICLE I. — *The Cervix Uteri, Before, During and After Labor.*

Read July 14, 1873, before the Chicago Society of Physicians and Surgeons, by JOHN BARTLETT, M.D.

In many of the text books on obstetrics the description given of the cervix uteri before, during and after labor is meagre, indefinite or erroneous. I propose this evening to give my views of its relations, with the conviction that if they are incomplete and not free from error, they are yet in the direction of truth, and with the expectation that this imperfect effort may lead to a full investigation and elucidation of the subject by those best qualified to do it justice.

I will first give the impressions which reading had left upon my mind as the commonly received relations of the cervix; then state the views which observation has suggested to me as correct; and finally, point out the interest or importance which these may have for the obstetrician.

As to my early views, which, it will be inferred, I deem as fairly deducible from the statements to be found in the books: in pregnancy, I regarded the cervix as remaining undeveloped until



CERVIX UTERI AFTER DELIVERY.

1. Cavity of uterus.
2. Firmly contracted os internum.
3. Flaccid tissue forming ring of cervix.
4. Cavity of neck.
5. Flaccid and flabby os externum.
6. Vagina.

within two weeks of term, contributing nothing to the cavity of the gravid uterus. That at that time it began to expand from below upwards, gradually disappearing from the touch, till nothing remained of it but a cushiony circle, or, at most, a narrow, flattened, sphincter-like ring of tissue.

Observing dilatation of the os and retraction of the tissue forming it to be coincident and allied phenomena, I naturally associated with the complete dilatation of the os the entire disappearance of the neck; so that, at the instant of complete dilatation, in the language of Cazeaux, "the uterus and vagina formed but a single uninterrupted canal."

After labor, I thought that the contractions of the uterus, taking place equally in all parts going to make up its walls, formed it into a body the shape of which, exteriorly and interiorly,

was not materially different from that of the distended condition. I believed that the margins of the os uteri which I had last felt still expanding and vanishing above the descending head, had kept pace in contraction with the body of the uterus, and that its firmly contracted, thick and rigid margin could be felt after labor immediately continuous with the upper end of the vagina. In time, according to my idea, as involution progressed, the uterine cavity

reached by the finger beyond the os just referred to, gradually contracted, and became divided into body and neck, as before labor; the os described meanwhile contracting, and assuming substantially its original shape and relations below the vaginal attachment.

These views I now regard as entirely erroneous. For them, observation has substituted these which follow:

In regard to the cervix before labor, I look upon all that part of the uterine walls projecting in vertex cases, like an inverted dome, into the vagina, and which may be felt enclosing the head, as the largely developed vaginal portion of the neck; that is, I regard all that uterine tissue lying below the attachment of the vagina, and which may be traced by the finger moving in any direction from the os uteri toward the vaginal circumference, as the vaginal portion of the cervix.

These statements, it must be noticed, have reference to the vaginal portion of the cervix only. There is reason to suppose that the uterine two-thirds of the neck are developed in like proportion. The extent of this development may be in a measure estimated by conditions or events accompanying natural and unnatural states of the womb after delivery.

I regard the vaginal portion of the cervix, at the time of the passage of the fœtus, as a collar of appreciable depth not in the same plane with the vagina, but projecting into it.

After the birth of the child, this fleshy band does not contract and form with its free margin the rigidly contracted orifice discovered after natural labor and generally somewhat indefinitely called the "os." Its tissue differs from that of the uterus, its power of contraction is much less. As a result, after delivery it may be felt as a flabby, floating collar hanging from the uterus into the vagina, "like a section of large intestine." Continuous with this fleshy cylinder, externally and above, is the attachment of the vagina; continuous with it, above, and within, is the rigidly contracted os internum. The involution of the post partum cervix soon begins, and at the expiration of ten or twelve days it will have assumed its ordinary form and relations.

As it was from examinations per vaginam, after labor, that this state of the cervix was made known to me, and as by this means inquirers will seek to verify these statements, it may be well to

make a detailed reference to such an exploration. If, after delivery, the uterus being contracted, a finger be introduced into the vagina, it may pass directly up to the "os," without coming in contact with any structures contravening the idea of the simple relation of the parts at first thought by me to be correct. If an examination be made critically, it will be recognized that the finger encounters an illy-defined orifice or slit, the margins of which are soft, flaccid and easily movable; penetrating these folds, the os is felt higher up than the normal position of the os externum by about the length of the floating cervix. The end of the finger resting upon this contracted ring, the os internum, the edges of which project prominently and abruptly inward, is not of course in the vagina, it is in the canal of the cervix. If it be moved outwardly from the pelvic axis in any direction, it will encounter a loose tissue which may at first be supposed to be the walls of the vagina; but if this surface be followed downward for an inch or an inch and a half, a free margin will be reached, which, upon being traced, will be found to be the rim of a fleshy ring. The surface encountered is the inner wall of the canal of the vaginal portion of the cervix; the margin traced is the still widely expanded, imperfectly contracting os externum.

The dimensions of the vaginal portion of the cervix are variable. A radius or meridian of the ante partum cervix, measured from the os to its circumference, is generally not less than two inches, and occasionally, when the mouth of the womb is far removed from the pelvic axis, the larger radii would measure five inches or even more. During the passage of the child, the length of the neck, usually about three-quarters of an inch, in some abnormal conditions is so great that its margins may be protruded at the vulva. After delivery, the infra vaginal cervix, commonly an inch, or an inch and a half, in length, in cases of extreme hypertrophy of the neck, has been found longer than the vagina.

The facts here detailed have a bearing upon several points of obstetrical theory and practice. The different action and condition of the body and neck of the uterus here pointed out may be considered another striking evidence of the correctness of the distinctions which physiologists and practitioners have drawn between these portions of the womb.

The views here given are important as bearing upon the long

disputed question of the mode of development of the cervix in pregnancy. On this point, obstetrical writers seem to be divided between two opinions; one set maintaining that the neck develops in expanding from above downward, and becomes part of the uterine cavity as early as the sixth month. Others, as Stoltz, Cazeaux, and Chailly, pronounce this view an error, and declare that the neck forms no part of the uterine cavity till within two weeks of term, when it expands from below upwards, disappearing gradually, and at the end remaining only as a cushiony circle.

In the light of the views here expressed, the second of these theories, which I believe is now most favorably regarded, is altogether erroneous; and the first fails to point out the whole truth. I think that early in pregnancy the cervix begins to develop throughout its entire extent; that gradually the upper portion of it expands and becomes a part of the uterine wall. Meanwhile, (and this is the fact which I think is entirely overlooked by Stoltz and Cazeaux), that portion of the neck which remains unexpanded has grown, as it were, on account of its *preparatory* development, to the length of the original cervix; finally, the whole is encroached upon and taken up to form the uterine walls. To put the idea in different language: early in pregnancy the neck is called upon to supply its quota to the enlarging body. Speaking somewhat figuratively, as ring after ring of tissue is demanded from the upper part of the cervix, the preparatory development in the remaining portion is such that the length of the neck is not apparently impaired, so that, what remains of it as late as two weeks before labor has been mistaken for the entire infra and supra vaginal cervix, whilst the loss by the continual transfer from the upper portions of the neck to the uterine walls has entirely escaped notice. That circle of the neck which corresponds at the time of an examination to the limits of its expansion, is regarded by writers as the os internum. The os internum is of course, as before labor, above the attachment of the vagina, and, near term, far removed from the examining finger. The apparent constriction taken for it, is simply that point in the cervical walls, marking the constantly descending line of demarkation between the expanded and yet unexpanded portions of the neck.

In my early examinations the contracted post partum os was mistaken for the os which I had felt before labor, the os externum;

and all that structure, consisting, at the beginning of labor, of the vaginal portion of the cervix, measuring several inches in depth, and forming the os which I had so patiently watched a short time before, was entirely overlooked and lost sight of, and by a species of jugglery as it were, a second os, which I knew nothing of, was imposed upon me for the one with which I had become from long watching familiar.

It is important, in lacerations of the os uteri, to know the site of the bleeding surface. In some cases of hemorrhage, occurring in the earlier years of my practice, which by a process of exclusion were referred to a laceration of the neck, I had no other idea than that the oozing proceeded from the little irregularities in the contracted os internum, when in all probability there were surfaces bleeding, such as I have since recognized under like circumstances, in the torn cervical rim, as large as the lower half of the little finger. In cases where the hemorrhage is referred to such lacerations, the practitioner will do well to trace the margin of the os externum; if it be much torn, the rent cannot escape detection, and the application of styptics may readily be made.

Some authors refer irregularities of outline of the os tinæ in multiparæ to lacerations of the mucous membrane of the os during labor. Is it not much more probable that such irregularities are dependent upon more decided lesions of the neck? My experience would indicate that lacerations are more frequent than is commonly supposed. The cases of hemorrhage proceeding from these injuries of the cervix which have fallen under my notice, have yielded to remedies intended to secure contraction of the uterus. As the mass of the vessels supplying this part of the womb pass through the tissue of the os internum, it is manifest that the want of contractility of the neck is in a measure compensated for by the mechanical constricting of its vessels by the contraction of its upper orifice.

In certain forms of rigidity of the cervix the relations here given would suggest an early resort to the knife. In those cases in which labor is delayed by the os girding the neck of the child, in all probability, as suggested by some writers, the constricting orifice is the os internum. In this connection, this inquiry has occurred to me: Does mechanical irritation of the os externum tend to excite contraction of the os internum?

The idea here presented of the mode of development of the cervix is of interest in relation to placenta prævia. By some, doubtless, it may be objected that the presence of the placenta over the os uteri is *prima facie* evidence that my opinions as to the development of the neck are incorrect. To my mind, there remains but the acceptance of the palpable facts, and the recognition in them of another striking example of the extraordinary power which nature has to accommodate herself to circumstances. The imperfectly contractile character of the tissue of the cervix deserves more attention than it has yet received in considering the growth upon it of the placenta; and it is possible that some fact of practical value in regard to the management of placenta prævia may be adduced from an improved knowledge of the nature of the tissue to which it is attached. By even the most recent writers, as Barnes and Thomas, it is distinctly stated that the os internum is below the placenta prævia; according to my views, the placenta is below the zone of fibres developed from and representing the os internum. In accordance with the theory of cervical development here stated, the attachment of the placenta to the cervical zone is the more recent, and, inferentially, the less firm in a line and direction *from* the os *toward* the placental margins, a circumstance indirectly favoring the suggestion of Barnes to detach the placenta from the zone of the neck in certain cases. Consideration of the statement above made in regard to the contraction of the fibres of the os internum as a cause of arrest of hemorrhage from the lacerated cervix, may confirm and elucidate the proposition of Barnes, "that the one constant condition of physiological arrest of flooding [in placenta prævia] is contraction, active and tonic, of the muscular structure of the uterus."

There is another condition upon which the facts here presented have a direct bearing. I refer to hour-glass contraction of the uterus. Before proceeding to any remarks on this subject, I will call your attention to certain considerations in regard to the position of the os internum. In the contracted state of the womb as stated, we find it in deep reach of the finger, and but little above the attachment of the vagina. But it must be remembered, that the relative altitude above the os externum of those fibres of the walls of the expanded uterus, the contraction of which forms the internal os, is not ascertained, and that it must vary with the degree of

expansion or contraction of the womb. Thus, in a flaccid condition of the organ after delivery, it may be several inches above the vaginal junction. The statements of writers as to the seat of hour-glass contraction, and the part of the uterus by which it is caused, are discrepant and confusing. Much of this misunderstanding and obscurity, it is believed, may be cleared away in the light of facts contained in this paper. From a person proposing to teach the true nature of this abnormal condition of the uterus, an answer to these questions should be expected: What was the condition in your cases of the os externum? Of the cervix uteri? Of the os internum? What was the probable altitude of the os internum above the os externum? And where, relatively to the internum, was the stricture?

I may state in this connection that, as the womb is capable of becoming flaccid as late as two weeks after labor, the os internum may be expected to be liable to corresponding changes after a like interval.

I desire, before closing this essay, to make some suggestions intended to account for the want of more general recognition of the correct anatomy of the cervix after labor. Its true condition has escaped the observation of many, because there is not very often occasion to make examinations which would disclose it. For the most part, after delivery, the fingers are introduced to ascertain if the patient be well cleared, and if such be the case, no further examination is made, or, in the event of the exploration of the vagina being again necessary, it is generally rendered so by an accident unfavorable to critical exploration. Again, it must be remembered that the condition here pointed out is dependent upon, and not to be recognized without a contraction of certain parts of the womb.

I have been asked, why has not the peculiar anatomy which you describe as distinguishing the post partum cervix been observed by the pathologist? I repeat, that the distinction indicated is dependent upon a vital contractility; when that passes away, the distinction ceases.

A few words more. It will doubtless be inferred that I regard these views as novel. Whilst it is manifest that the true condition of the cervix uteri during and after labor is practically familiar to many standard writers, so far as my knowledge of the literature

of the subject goes, no one has connectedly set forth the facts here stated.

I am well aware, however, that my means of reference are too limited to justify me in saying that they are not well known. Of this only am I assured, that however often these ideas may have been taught, a lesson from them is not now amiss.

ARTICLE II.—*Case of Chronic Inflammation of the Stomach.* From the Transactions of the Chicago Society of Physicians and Surgeons; Meeting of June 14, 1873. By Drs. A. FISHER, JAMES N. HYDE, and I. N. DANFORTH.

Dr. A. Fisher gave the following details of a case of chronic inflammation of the stomach:

I have been acquainted with Mrs. I——, for the past sixteen years, and during that time have been her only physician. She was 49 years old, of a nervo-bilious temperament, and was always in delicate health, though never dangerously ill. She suffered from occasional attacks of intermittent fever, and generally had a red tongue, irritable stomach and dyspeptic symptoms, though she was able to discharge her household duties, and called herself well.

The following note, penned by her husband, describes her later condition:

"In the summer and fall of 1872, she complained frequently of a 'fullness' after eating, or very moderate drinking, and remarked several times, laughingly, 'if it were not absurd, I should say my throat was filling up.' During January and February of 1873, this feeling increased, and, towards the latter part of February, she began to lose her usual capacity, though not her desire, for food. Believing herself to be suffering from an aggravated form of dyspepsia, she tried very many remedies without success. In March, she gradually commenced the use of gruels, etc., as the only dietetic articles she could retain on her stomach, and was soon visited by that physical weakness which results from a low diet. All through March and early in April, she repeatedly remarked, 'I must get up, for I feel that if I should keep my bed for one day, I should not soon want to get up again.' On the 9th

of April, when we moved, she was extremely weak and emaciated, and, on the 12th, was startled by your asking if she had any hereditary tendency to cancer in her family. On the 20th I described her condition to a friend as follows: Parched mouth, with coppery taste and canker sores—copious discharge of frothy phlegm—great thirst—burning pain in the chest—distress always after eating, sometimes after drinking, located under the breast-bone, and between the breasts—tendency to vomit food directly after eating, and, when the latter was retained, free vomiting of frothy matter—regular pulse and constipated bowels. On the night of June 6th, she commenced to discharge a fetid yellowish matter at intervals of about six hours. At ten o'clock A. M. of the 9th, this discharge ceased, but recommenced at seven o'clock the next morning, and was very copious, but not nearly so fetid as before. Her average pulse during the week that preceded her death, was 112—up to June 9th, the highest noticed was 122. From the 4th to the 10th of June, she had no stools, but frequent copious and painless urination. She had no *feeling* of fever, though numerous signs of its presence were not wanting. On the 10th of June, morphia was administered at eight o'clock A. M., and six P. M., at her urgent request; she was then failing fast, and died quietly at four o'clock A. M., June 11th."

Mrs. I—— called on me for a prescription last October, (I had not prescribed for her previous to that time for nearly one year), and said she thought her liver was affected. I made a careful examination, and then informed her that her stomach was the source of her trouble, and advised her to regulate her diet. I ordered for her bismuth and pepsine, which she thought for a time gave her relief. I prescribed for her two or three times in October, once in December, and three or four times in March, using those remedies which were indicated to allay the irritability of the stomach and to assist the digestion—this without any permanent benefit, the patient growing weaker and becoming more emaciated.

I was first called to see her in her last illness, on the 13th of April. I found her very much emaciated, with sallow skin, dry, red and smooth tongue, pulse 70 in the minute, with tenderness in the region of the stomach, and an irregular indurated mass, evident to the touch somewhat to the left of the pit of the stomach. She com-

plained of a burning pain under the middle of the sternum, aggravated by the ingestion of food, which she did not believe entered the stomach. She took no solid food, and but a few spoonfuls of liquid nourishment at any one time, this amount generally producing vomiting of a glairy, frothy mucus, unmixed with either blood or pus. Her bowels were constipated, but moved daily by enemata.

I ordered eight grains of the subnitrate of bismuth every six hours, with sinapisms over the stomach, and a diet of beef-tea and mucilaginous drinks. In four days I saw her again; there was little less vomiting, and she was otherwise in no better condition. I then prescribed a pill of one-fourth of a grain of nitrate of silver, and two grains of the English extract of *cicuta*, every six hours, continuing at the same time the use of the bismuth—this with apparent benefit for one week. The "burning heat" was not so severe, though she felt that nothing remained in her stomach and had occasional attacks of vomiting.

On the 25th of April, Dr. N. S. Davis saw her with me in consultation. We agreed that there was thickening and contraction of the stomach, and, from the feel of the noded induration, already described, as well as from the consideration of the history of the case, we were led to fear cancer of the stomach, but did not express a positive opinion on the subject.

Dr. Davis advised the continuation of the nitrate of silver and *cicuta*, and the substitution for the bismuth of one-fourth of a grain of carbolic acid, with four drops of the tincture of belladonna in syrup every six hours. This course was followed for one or two days, when either the carbolic acid or the belladonna irritated the stomach, and both were suspended; the pill being administered alone for about one week longer. At the expiration of that time, all medication was discontinued, as she could not retain even a teaspoonful of beef-tea in the stomach. Her mouth and lips were kept constantly moistened by water, and a teaspoonful of it given when called for.

Her sufferings were very great and grew constantly worse throughout her illness, and as a cure seemed impossible, I advised, at the last, hypodermic injections of morphia sufficiently often to relieve her of pain. They had a happy effect, and she continued to take them, increasing the dose according to the necessities of her case for about three weeks, until she died, June 11, 1873.

The autopsy was made by Dr. James N. Hyde, June 12, 1873.

There was great emaciation and cadaveric rigidity. The brain was not examined. The heart, lungs, liver, spleen, kidneys, pancreas, uterus and ovaries, were found to be in a condition entirely normal. The stomach was greatly contracted, especially at a point about one inch distant from the pyloric extremity, its walls were hypertrophied, its mucous membrane congested, thickened and partially eroded. There was no post-mortem digestion of its coats. A small cul-de-sac, on the left side and in the greater curvature, seemed to exhibit fewer traces of inflammatory products than any other portion, and was partially filled with a serous liquid. The œsophagus was not implicated in the diseased portions. The omentum was studded with small granules of the size of split peas, and, in the interstices of the latter, was moderately injected, but not greatly altered. The duodenum seemed quite free from any thickening of its coats, but, to a certain extent, participated in the condition which was evident in all other portions of the intestinal tract—congestion and traces of a low form of inflammatory action being everywhere present. The bladder was moderately distended with limpid urine.

The stomach was sent to Dr. I. N. Danforth for microscopical examination.

PATHOLOGY.

The walls of the stomach were thickened to an extreme degree; the organ was very much contracted, in fact, it was gathered up into a hard, irregular, "puckered" mass of folds and ridges, and was doubtless permanently retained in this condition. The mucous membrane was excessively hypertrophied, rough and brawny; it was successively elevated and depressed, as a consequence of the general contraction of the stomach, and these elevations and depressions, or "hills and valleys," (*Rindfleisch*), remained after the stomach was cut open and put upon the stretch.

MICROSCOPY.

I have examined several thin and carefully prepared transverse sections, extending through the entire thickness of the stomach, and find one uniform condition applying to them all.

First.—The connective tissue is enormously increased, not only

the submucous layer and that beneath the muscular tissue, but also that delicate inter-connective tela which is associated with the gastric follicles. This new tissue is of course the product of an inflammatory proliferation of the connective tissue-cells; these cells, being stimulated by inflammatory action to an abnormally rapid growth, must necessarily produce tissue which is abnormal in *type* as well as in quantity, in accordance with the general law governing inflammatory proliferations. Indeed, this new tissue so nearly resembles ordinary cicatrix tissue, both in appearance and behavior, that we may regard them as practically identical.

Second.—The gastric follicles are all but destroyed; they have been slowly choked and squeezed out of *functional* existence by the progressive contraction of the newly formed connective tissue. In none of the sections examined, were the gastric follicles in anything like the natural size or condition, and generally they were so pinched and distorted as to have almost lost their identity. Hence, the inability to retain or digest food. But the same contraction which destroyed the gastric follicles, also involved the nerves of the stomach; they were subjected to exactly the same mechanical violence, and of course they uttered their protest in the only language known to them. Hence, the extreme pain of which the patient complained, and the resulting inference that the case was possibly one of cancer of the stomach.

We have, then, this sequence of events:

1st.—The development of inflammatory action, which was from the first of low "grade," and therefore all the less likely to attract sufficient attention, and cause sufficient discomfort to lead the patient to call a physician, or seek relief from a trouble which, in its incipency, was probably regarded as hardly worth noticing.

2nd.—The proliferation of connective tissue-cells, producing a superabundance of tissue of low type, which so nearly resembled scar tissue that it behaved *like* scar tissue. Hence, the extreme and permanent contraction of the stomach. It is noticeable that the tissue primarily involved, namely, the connective tissue; and the pathological condition produced as a consequence thereof, namely, contraction, are precisely analogous to the events and consequences which we meet with in cirrhosis of the liver, and the small contracted kidney of one of the forms of Bright's disease.

ARTICLE III.—*Special Meeting of the "Chicago Society of Physicians and Surgeons," Monday evening, August 18, 1873.* Reported by PLYMMON S. HAYES, M.D., Chicago.

The meeting was called for the purpose of continuing the discussion of the subject of cholera. Dr. Owens was called to the chair.

Dr. Simons made the following verbal report: There had been ten deaths from cholera since the last meeting. The temporary hospital, situated near Burnside street, between 33rd and 35th streets, had been opened Tuesday, August 12th. A number of patients had been admitted, and these, when not in a stage of collapse at or about the time of their removal, appeared to enter upon convalescence more speedily than when treated at their residences. Carbolic acid, sulphate of lime, and chloride of iron, had been largely used as disinfectants. Five cases were in the hospital at date, and all but one were progressing favorably. Two of the nurses had exhibited premonitory symptoms of the disease, but soon recovered.

Little attention had been paid by the inhabitants of the infected district to the circulars distributed by the Board of Health. Even when summoning a physician to patients in collapse, they generally declared that the existing disorder was nothing graver than a simple diarrhœa.

One-third of the inhabitants had left the infected locality, and in spite of all remonstrance, few of the Germans could be induced to destroy their feather beds upon which cholera patients had died. Upon removal they generally carried with them these beds, and such elements of contagion as were capable of transportation therein.

Dr. Wood had examined under the microscope some water obtained from a filter well at No. 947 Burnside street, south of the city limits, and presented a specimen of it to the Society. The well was ten feet below the surface, and contained, therefore, surface water merely. The water had been filtered through two layers of sand and one of charcoal. Upon applying the permanganate of potassa test, he estimated that it contained 91,279 grains of oxidizable organic matter to the gallon, consisting mostly of a disorganized vegetable detritus, evident under the microscope.

Dr. Simons remarked that the majority of the recent cases had not made use of well water, but of hydrant water. Public hydrants had been that day established as far south as 37th street, the city limits, and the hydrant water had thus been placed within easy access of the entire neighborhood.

Dr. Boyd stated that the first cases of cholera in the section referred to, occurred in a family which had resided there for two years—the father and son were laborers, and their fare was extremely meagre. Every member of the family died; and he considered that every subsequent case was more or less directly traceable to this source of infection.

Dr. Wilder reported a case on 20th street, which Dr. Chas. Gilmore Smith considered to be a case of death from dysenteric hemorrhage.

Dr. Hyde read a letter from Dr. Davis Halderman, of Columbus, Ohio, giving details of fifty cases of cholera with twenty-seven deaths. The disease first appeared just outside the penitentiary, and the cases were confined to those living on low grounds. No symptoms of the disease had occurred in the infirmary, where, for several reasons, it might have been expected.

The speaker had secured a "rice-water stool" from Dr. Boyd, and had forwarded it under seal to Dr. I. N. Danforth for microscopical examination. The latter had just telegraphed that he had been unavoidably detained from attendance.

On motion, it was resolved that Dr. Danforth be requested to make known the results of his researches at the next meeting.

Dr. Simons stated that the following formula, obtained from Dr. T. Bevan, a member of the Society, had been employed with success:

R.	Acid Sulphurosi,	-	-	-	-	-	oz. j.
	Magnes. Sulphitis,	-	-	-	-	-	dr. j.
	Capsici Tincture,	-	-	-	-	-	oz. ss.
	Aquæ Destillat.,	-	-	-	-	-	oz. j.
M. et S.	Cochl. parv. no. j. in cyath. aq. bis die.						

This preparation was intended as a prophylactic. When used as a remedy, two grains of morphia were added to the above, and a teaspoonful exhibited in water every half hour.

The following committees were then appointed, with instructions to report fully as soon as practicable:

"Committee upon the history, nature and contagious character of the disease existing at present near the southern borders of the city."

DR. THOMAS BEVAN, *Chairman.*

DR. JNO. BARTLETT.

DR. I. N. DANFORTH.

DR. C. J. SIMONS.

DR. LESTER CURTIS.

"Committee on the most appropriate prophylaxis and treatment of the disease."

DR. J. H. ETHERIDGE, *Chairman.*

DR. H. P. MERRIMAN.

DR. U. M. BOYD.

DR. CHAS. GILMORE SMITH.

DR. F. H. DAVIS.

On motion, it was requested that hereafter the Secretary of the Society supervise the reports prepared for publication of its transactions in the secular journals.

The Society then adjourned.

ARTICLE IV.—*A Case of Locomotor Ataxy, Benefited by the Use of Nitrate of Silver.* By G. C. PAOLI, M.D., Chicago.

Mr. B., 39 years of age, formerly a grocer, requested me to call and see him, in the beginning of the month of April. He informed me that, previous to my visit, he had been sick about eighteen months. On examination, I found the patient's appearance was pale, and he was somewhat depressed in spirits; his intellect was still unimpaired; his appetite was tolerably good, but he was inclined to vomit after a hearty meal, and also complained that he felt drowsy and heavy after he had eaten a good dinner. The bowels had a tendency to be confined. The bladder was rather weak, and sometimes the urine was passed involuntarily, accompanied with pains. There were no pains along the spinal column on pressure, except on the lower part of the vertebra lumborum. The upper extremities were apparently well nourished. The motor power of the arms was not impaired, except that when I asked him to press my hand, I found he did not possess enough strength to produce any powerful grasp.

The lower extremities appeared in a healthy condition. Temperature normal; the sensibility of the skin of the right leg somewhat obtuse. When I asked him to rise from his chair, he did so with extreme difficulty, and experienced equal difficulty in walking, staggering like a drunken individual; he lifted up his legs, but

threw them about in a disorderly manner. His sexual power was entirely extinct.

He admitted to me that he had abused himself in practicing masturbation, and had worked very hard to obtain an independent position; and, as I understood, denied himself regular and substantial meals, with the idea of getting rich.

He further informed me that his attention was first called to the disease, on finding himself gradually growing weaker in the lower extremities, and shooting pains in his legs, which were aggravated in wet weather, and sudden changes of temperature.

My prognosis in the case was rather gloomy, and I informed him that I could scarcely benefit him. I prescribed nitrate of silver, in half-grain doses three times a day. This remedy was first suggested and used by Professor Wunderlich, of Leipzig, and cases are reported by him which have been cured. Other physicians have also used it, but have not experienced any beneficial effects. In this case it quite surprised me: after using it about one month and a half, the patient could walk with a steady gait, and his appetite and digestion very much improved.

The pathology of this peculiar disease is very obscure at present. It has received a new name—locomotor ataxy—from Dr. Duchenne, to whom we are indebted for a graphic description of it; but to others belongs the merit of scientific researches, though the disease has been described under a different name—tabes dorsalis. It was in the year 1827, that Dr. Horn, of Berlin, gave a very good description of the symptoms, and later, and previous to Dr. Duchenne's description, Dr. Romberg presented some interesting details of the disease under the name of tabes dorsalis; but neither of the above physicians have, up to the present time, given any satisfactory explanation of its pathology.

ARTICLE V.—*Iodide of Potassium in Erysipelas.* By A. H. HIATT, M.D., Wheaton, Ill.

Having used iodide of potassium in the treatment of erysipelas for the last ten years with the best results, I deem it right to lay the fruit of my experience upon this subject before the profession, and the more so, as I do not see this remedy for erysipelas

recommended, or even referred to in any of the works on the practice of medicine which I have read.

The variety of the disease upon which I base my remarks particularly, and present my treatment, is the phlegmonous, though the remedy is equally efficacious in other varieties in which I have tried it.

It is when the disease makes its appearance first upon the side of the nose or the face, passing rapidly over the contiguous parts, until the entire face and scalp are invested, obliterating the features, enormously enlarging the head, the patient presenting a hideous and ghastly appearance, that the effects of this remedy are most promptly and happily seen. When called to such a case, whether the disease has passed to the full extent above named, or has but just commenced its ravages, I immediately prescribe the following :

R. Potassii Iodidi,	- - - - -	dr. j.
Aqua Pura,	} - - - - -	aa. oz. j.
Syrup Simplex,		
Ess. Gaultheria,	- - - - -	dr. ss.

S. One teaspoonful every two hours, mixed with water; equal to seven and a half grains per dose.

This I continue until the violence of the disease is subdued, as indicated by abatement of the swelling, and mitigation of the tenderness of the tumid parts upon touch—and afterwards I continue the medicine in less quantity, or at longer intervals, until all traces of the disease are gone. In some very severe cases, attended with delirium, I give as much as ten grains of the iodide every two hours.

If the bowels are constipated, and the tongue has a brown coat, I administer fifteen grains hyd. cum creta, and follow it in six hours with seidlitz powder or citrate magnesia—a portion to be repeated every two hours until the bowels are moved.

For the prostration and failing strength, I give about two grains of quinine every four or five hours, or spts. vini gallici or old Bourbon whisky in tablespoonful doses every two, three or four hours, as the case may require.

I also insist upon the use of milk, animal broths, or other forms of nourishing food, from the commencement. If these cannot be taken, beef essence must be used.

As an external application to the inflamed and swollen parts,

I have found the following simple preparation the very best for its soothing effect merely, for I am convinced that no external application exerts any curative influence over the disease :

R. Plumb. Acetas, - - - - - dr. j.
 Aqua, }
 Glycerine, } - - - - - aa. oz. j.

The parts to be kept moist with this.

In from twenty-four to forty-eight hours, under this treatment, the violence of the disease is usually subdued.

I can present a long list of cases treated as above, all terminating favorably, a complete recovery being effected in from two to four or five days in every instance—and which results I have seen under no other treatment.

I regard the external application of blisters, nitrate of silver, tincture of iodine, corrosive sublimate, and all other irritating and corrosive substances, as not only useless, but positively injurious, inflicting unnecessary and severe punishment upon the patient, and wasting the strength as well. I do not believe in specifics in medicine, and yet I regard iodide of potassium in erysipelas as approximating one as nearly certainly as quinine in intermittent fever.

If in the above article I shall succeed in directing the medical profession to the faithful trial of this remedy in this affection, I shall feel I have rendered invaluable service to my race.

OFFICE, 125 CLARK ST., CHICAGO.

ARTICLE VI. — *A Case of Death from Umbilical Hemorrhage.*

Reported by NORMAN BRIDGE, M.D., Chicago.

The following notes are submitted, as adding one more case to the list of deaths from an unusual accident.

Mrs. F., aged about 42, in appearance large and robust, was delivered of her fourteenth child on January 3rd, 1873, after a labor of less than three hours. It was her belief that the time was a month short of full term. The child was small, but seemed healthy; it thrived until the 10th inst., the cord having shriveled and dropped off a day or two before.

I had not seen the case for several days, and was called hurriedly on the morning of the 10th, to find that the child had been bleeding from the umbilicus for six or more hours. All the bandages and clothing were saturated with blood, indicating the loss of several ounces. The child appeared thoroughly exsanguinated, and had a rapid and irregular pulse.

Powdered per-sulphate of iron was applied to the bleeding surface, and a thick compress bandaged tightly against it. There was ordered frequent doses of brandy.

No more bleeding occurred, but the child died during the afternoon.

The surface of the umbilicus was so much retracted that the particular vessels yielding the blood were not made out.

ARTICLE VII.—*J. Payne, D.D.S., on Amalgam Plugs.* By BURKE PILLSBURY, M.D., Duluth, Minn.

In the July number I find an article on "Poisoning from corrosive sublimate generated in the mouth from amalgam plugs in the teeth. By J. Payne, D.D.S." Your correspondent, it seems to me, takes a good deal of credit to himself for knowledge, which it is hardly to be expected that he, not being a medical man, should possess, and which in fact he does not seem to possess. When he makes such broad assertions he should offer some evidence of his acquaintance with the subject of which he treats, and while condemning an artificial filling in an old tooth as the cause of all earthly diseases, give us some satisfactory starting point. He mentions "twenty cases" he has had, "nearly all pronounced by physicians as consumption," leaving us to infer that they were otherwise, and so discovered by his superior skill and understanding. It would be interesting to his readers to know what means he and his rivals used in making their diagnoses. His symptoms are, "at times a very bad cough, eyes sunken, and haggard expression and deep blue or dark color under the eyes, invariably a metallic taste in the mouth, water flowing from the mouth in the night while asleep so as to wet the pillow, and in most cases extreme prostration"—any of which symptoms are common

attendants of disease, and mark nothing in particular. What person has not, in perfect health, while dreaming of food, had saliva flow from the mouth more or less copiously, and not possessing a single amalgam filling?

Then his theory of bichloride of mercury being formed from the decomposition of the salts of the saliva, is pretty far-fetched, and would require all Liebig's skill in the demonstration. Chloride of sodium and potassium exist in the saliva in the proportion of *eighty-seven one-hundredths of one part in a thousand*, and the proportion of mercury that would escape, if at all, would be so infinitely minute that a microscope aided by the faith of a homœopath would not discover it. In addition to this, any one acquainted with chemistry knows that a chloride and mercury will not combine by any such simple process as contact and a little warmth.

Selections.

Cancer of the Female Breast—its Character, Diagnosis, Prognosis and Treatment. Medical Library and Journal Association:
Dr. JOHN C. PETERS, Pres., in the Chair.

PROF. WILLARD PARKER remarked as follows:

In the latter part of the last century the man who was particularly connected with the study of this subject, cancerous tumors, was Dr. John Abernethy. Previous to his time, our knowledge upon this subject was in a state of confusion. So completely and successfully did he study this subject of tumors, that, in his time, the results of his investigations were regarded as the *ultimatum*. They were so generally accepted by the profession, that whoever was familiar with what Mr. Abernethy said upon the subject, was supposed to know all that could be known about tumors. These results of the investigations of Mr. Abernethy continued to control the minds of the profession, to a very great extent at least, until perhaps the last twenty-five years. Since that time great changes have taken place, which have grown more particularly out of investigations arising from the use of the microscope and from the researches of the histologists.

The first great departure was introduced by Bichat.

Bichat was the father of general anatomy, and was one of the brightest examples of a thorough scholar that France has ever pro-

duced. It was by the influence of his study that the human body was divided into its different systems—the muscular system, the vascular system, the nervous system, etc. In connection with his great work, the study of general anatomy, he took up the study of tumors. In his studies upon that subject, he observed that tumors were growths in the body, abnormal in character, that some of them had a character analogous to the original natural tissues, and that others varied somewhat from the original natural tissues. He observed, therefore, that there were two classes. One *like* the original normal structure, and the other *unlike* the original normal structure, and had a structure *sui generis*.

Bichat died in the year 1802. His pupils, Dupuytren, Andral, Cruvelhier, and others, seized upon this idea which Bichat had advanced, and made the two great classes of tumors with which the profession has been so familiar since their time. They divided tumors into two great classes, one which embraced that class of tumors which were composed of a structure analogous to the natural structures of the body, which they called homologues or homoplasts, and another which embraced that class of tumors which were composed of a structure which found no analogy in the body, which they called heterologues or heteroplasts. They farther observed that the first class were almost always mild in their effects, easily controlled by treatment, and they received the additional name of benign. They also observed that the second class were very apt to destroy life, sometimes very rapidly, were not easily controlled by treatment, and they received the name of malignant. This division of tumors was persisted in for a long time by the French, and it has been in France that the strong advocates of this classification have arisen. In the latter part of the life of Velpeau, that great surgeon began to question this matter of homology and heterology, as the terms had been used in this connection, but his studies and researches assumed no definite shape. The next class of men who attempted to give a solution to the question, What is cancer? were the chemists of France, and of these there were Thenard, Lassaigne, Vauqualin, and others of equal ability. These men made a careful analysis of cancer, with the view of discerning, if possible, something peculiar to cancer: something which perhaps might receive the name of *carcinomatine*, a distinct and fixed substance, the presence or absence of which might always enable us to say whether cancer was present or absent. After a long and careful research, they found that their labors were unsuccessful, and the work was allowed to pass from their hands, and it fell into the hands of the histologists. This class of observers have been diligent in their study, and faithful in looking for some change or modification of the cell-products, shape of cells, or presence of peculiar elements, by which they could distinguish cancer. For a long time it was thought that they had

succeeded in making the desired discovery, and the announcement was made that the essential element in cancerous growth was the presence of a certain shaped cell, which was immediately denominated the "cancer-cell." This announcement was very generally accepted by the profession as a solution to the long and vexing problem. It is only a few years since we all had faith that the so-called "cancer-cell," or morphosis, something distinct and definite in its character, which had been found, would enable us to determine whether any given growth was cancerous in its nature, or whether it was not cancerous in its nature. But the truth is, we now know that no one cell at present can be fastened upon as absolutely representing cancerous disease.

The "cancer-cell" theory belonged to the French.

The Germans, on the other hand, never acceded to this proposition, and for some time there was considerable discussion between French and German scholars upon this question. Among the Germans who engaged in this discussion were Virchow, Rokitsky, and others, who have since become pre-eminent in their profession. The study of this subject, however, has been followed up by Virchow particularly, and now we have the result of his investigations formulated and presented to us in his great work upon the pathology of tumors.

First of all he lays down the proposition that everything proceeds from a cell—

"Omnis cellula e cellula."

For a time his opinion was that the embryonic connective-tissue cell had the power of proliferating the cancer-cell, that is, that the connective-tissue cell could produce a cell like itself, and also a cell of a different character, which was the cancerous product. But it now seems to be settled that like only begets like, and the doctrine that the connective tissue is capable of proliferating a cell of a distinct character, which can be called the cancer-cell, is regarded as not being tenable.

The doctrine is now, that the true cancer, so far as we rely upon the cell, is made up of cells which are of the epithelial type.

Failing to discover a peculiar cell by which cancer was to be recognized, both histological and pathological observers have at present settled down upon the point that true cancer is to be recognized by certain peculiarities in its anatomical structure. That is, we have in the first place a stroma made up of connective tissue; that in the interstices of this connective tissue, denominated alveoli, are deposited cells; that these cells are of the large, nucleated variety, and have for their prototype the epithelial cell. The presence of this anatomical arrangement is now regarded as evidence of the presence of true cancer. In other words, that nearly all cancers, if not all, have their origin in epithelial structure. This portion of the subject is one of great interest, and it is

to be hoped that, at some future period not far distant, the histologist and pathologist, by aid of the microscope and other means which they have at their command, may be able positively to determine and settle the question as to what changes have taken place in the growths which we now call cancerous. Virchow, pursuing the study still farther, as relating to the clinical history of these growths, has given us several stages.

1. The stage of development or local irritation.
2. The stage of granulation.
3. The stage of differentiation. That is, from being an apparently homogeneous structure, some of the cells go on to make muscular tissue, epithelial structure, etc.

Of these varieties of cells he distinguishes four; that is, cells which are to appear in four different kinds of structure.

1. Connective tissue.
2. Nerve structure.
3. Muscular structure.
4. Epithelium.

Continuing the stages he makes

4. The stage of maturity.
5. The stage of retrograde metamorphosis, or the stage when it has attained its maximum growth and begins to retrograde and die.

This process may take place by ulceration, suppuration, adipose change, and so on.

This embraces all the general remarks I have to make on the question, What is cancer?

Now, with regard to cancer of the female breast. There is hardly any organ in the human body so frequently the seat of this disease as is the female breast. Of course when cancer involves any important organ in the body it becomes the object of great solicitude on the part of the patient and the friends, and on the part of the surgeon a source of great anxiety, and, I am sorry to say, without any satisfactory results attending his efforts to give permanent relief.

In the first place, with regard to its frequency. Upon this point we have some very instructive tables furnished us by a French surgeon, who made his collection of cases during the eleven years immediately subsequent to the year 1830. The whole number of cases he collected, of all kinds, was 9,119.

Of these, 4,222 occurred in organs peculiar to the female sex, and only 48 occurred in organs peculiar to the male sex. This is in the proportion of 88 to 1. The reason for this extreme difference in frequency in the two sexes is not known.

Of the 4,222 cases which occurred in women, 2,996 cases occurred in the uterus; 1,147 in the mammæ; 64 in the ovaries; and 15 in the vagina.

Of the 48 cases which occurred in men, 21 cases occurred in the testes; 10 in the penis; 7 in the scrotum; 5 in the prostate; and 5 in the pap.

A remarkable feature of these observations is that so few cases occurred in the ovaries.

I propose now simply to refer to the cases which have fallen under my own observation. These are cases which I have either treated myself, or have seen in consultation. These observations and notes extend over a period of about 40 years, but the larger portion of these cases have occurred within the last 20 years.

I have upon my tables the record of 295 cases.

In making up these tables I have taken into consideration the following points:

1, date; 2, name; 3, age; 4, civil condition—married, single, or widow; 5, number of children; 6, which breast was involved; 7, family history; 8, personal history; 9, diagnosis, prognosis, treatment; 10, subsequent history.

First, with regard to age:—Average age, $48\frac{1}{10}$ years; oldest, 86 years; youngest, 28 years.

Civil condition:—Married, 190; widows, 53; single, 34; unknown, 18.

Number of children:—Mothers, 186.

Breast:—Right breast, 107; left breast, 130; axilla involved, 71.

It will be noticed that there is a difference of 23 in favor of the right breast. I have also found that the same result has been obtained upon this point by other observers. I can assign no reason why its occurrence should be so much more frequent in the left breast than in the right.

Family history:—Cancer taint, 28; no cancer taint, 208; consumption, 35; consumption and cancer taint, 5; unknown, 36.

It will be noticed here that by far the larger portion occurred in persons who had no history of cancer taint. So far, then, as my tables go, they prove that cancer is not a hereditary disease, which is contrary to the opinion I had always entertained previous to a more thorough examination of the question.

Personal history:—Blows, 38; injury from breastpump, 1; injury from tight dress, 2; inverted nipples, 2; uterine sympathy, 19; mental anxiety, 45; commencing benignant (op. 17), 31; commencing malignant, but living long (op. 14), 15; sloughing and living long (op. 3), 6; cystic (av. age, $57\frac{1}{2}$ —oldest, 72, youngest, 33), 9; metastatic, 3.

With regard to the 19 cases which occurred from uterine sympathy, they belonged to that class of cases which formerly were called the irritable tumor of the breast. These cases not uncommonly occur in connection with dysmenorrhœa, and the leading symptoms are swelling and pain in the breast and nipples before menstruation.

I wish to call special attention to the 45 cases marked mental anxiety as a cause for their development. I believe this to be an important point in connection with the development of cancer in the female breast. I believe it is very rare for cancer to present itself where everything passes pleasantly and prosperously in life, and cheerfulness at all times surrounds the individual. I find in many cases, that the development of cancer dates at something which has caused mental anxiety, such as loss of property, loss of friends, long-continued sickness in families, giving rise to great mental solicitude and physical fatigue, etc., etc.

In addition to the above general features, I find that I have the record of 31 cases which began as benign tumors. In this class of cases the tumors had been carried from 10 to 30 years without inconvenience, but suddenly from some cause, mental anxiety being a prominent cause, these tumors commenced to grow rapidly and assume a malignant form. One of these cases I now recall, in whom the tumor had existed from early womanhood. That lady was married when 20 years old, bore two children, her husband was prosperous in life, but suddenly distress in life came from reverse in fortune; and during these dark hours in her history, the tumor took on changes which developed into a rapidly advancing cancerous growth. The tumor was twice removed, but no permanent relief ensued.

Another case was one in which the patient carried the tumor 35 or 40 years without inconvenience, when suddenly the tumor took on new action, and the woman died from the effects of cancerous disease within six years from the time of the manifestation of this new action.

I have another group of cases which began as malignant. They are 15 in number. Of these cases, 14 were operated upon, and remained non-progressive for a long time. These are the cases which have afforded the greatest success in treatment. The following is a condensed statement of the history of these cases subsequent to the operation:—

1. Lived 12 years, and died with cancer of liver.
2. At the end of 27 years was in fair health.
3. Lived several years, and died from accidental injuries.
4. Patient well 22 years after operation. The patient had an issue made in the arm.
5. Lived 36 years, and died of other disease.
6. Patient well 5 years after operation.
7. Patient well 8 years after operation.
8. Patient well 10 years after operation.
9. Patient was operated upon in 1856, disease returned in 1873, but is not active.
10. Living and well 20 years after operation.
11. Living and well 3 years after operation.

12. Lived 7 years after the first operation. This patient had the second operation.

13. Patient living in 1873, 10 years after first seen, and she had no operation.

14. Disease returned 2 years after operation, after great depression of spirits.

I have another group of cases which took on the sloughing process; those in which ulceration and sloughing took place rapidly and continued.

In this group are six cases. Of these, three were operated upon and three were not.

Of the cases in which no operation was performed—

1. Three years' duration, and patient lived 6 years after the sloughing commenced.

2. Nine years' duration, had been ulcerating 3 years, general health unimpaired.

3. Thirty-eight years' duration, and had been sloughing for 10 years.

Of the cases in which operation was performed—

1. Lived 5 years.

2. Operation 12 years ago, patient still living.

3. Patient well 10 years after operation.

This last case was indeed a most remarkable one. When first called to see the lady, I found her in what was apparently an exceedingly unfavorable condition. There was a large sloughing, cancerous mass upon the thorax, which filled the house with the most terribly disgusting odors which emanate from such sloughing masses. The glands in the axilla were not involved. There was no trouble above the clavicle. Operation was resorted to, simply to relieve the patient of the presence of this mass of death. There was no promise whatever of hope in the case. The operation was performed ten years ago, and the patient is still living and in good condition. These sloughing cases, when the surrounding parts are not involved, are to be regarded as favorable cases for an operation, rather than unfavorable. The fact of sloughing should have but little weight in deciding for or against the operation, but if anything, it is to be regarded as favorable.

Another group of cases of which I have taken note belongs to the so-called cystic variety.

Of these cases I have nine. Three of these cases were operated upon, but only one is now known to be living. In that case, cancer taint was present in the family. The patient is now living, ten years after the operation. What is meant by cystic cancer is, that they are tumors containing fluid, and the walls of these cysts or the walls of the tumor contain cancerous structure.

These cases are quite favorable for operation. The common cause for their occurrence is blows.

There is another class which has been denominated metastatic cancer. Properly speaking this term does not express what is understood by these cases. What is understood by the term is, that the disease has ceased its activity in one organ of the body and made its appearance in some other organ of the body. The organs most commonly affected in this way are the uterus and the mammae.

One case I call to mind, as illustrating this change, occurred in a woman who had cancer of the breast at the age of 54, which subsided in its growth, and at the age of 76 it appeared in the uterus, and was there the immediate cause of the patient's death. I have seen two of these cases in which the transition of the disease was from the breast to the uterus, and one in which the transition was from the uterus to the breast. As a general rule, the constitutional disturbance in these cases is but little, and the patients are generally able to attend to the common duties of life apparently in a very comfortable condition. I will now say a few words with regard to the

FORMS OF CANCER.

Of these we have, speaking particularly of the breast—

First—Those beginning as a tubercle in the skin.

Second—Those beginning as an ulcer on the nipple. These begin very much as does the epithelial cancer of the lip.

Third—Circumscribed. This form occurs more commonly in young persons, usually in fat persons, and is of rapid development. Sometimes this form involves one lobe, sometimes several lobes of the breast, and is something corresponding to the so-called encephaloid form.

Fourth—The infiltrated form, or scirrhus. This usually returns very soon after operation, very rapidly involves the lymphatics, and is seen very often in the skin. The disease makes its appearance in the skin in the form of small lenticular masses, sometimes called tubercles in the skin.

Fifth—Cystic. This form is usually found in persons past the middle period of life.

Sixth—The sloughing variety.

DIAGNOSIS.

Under this head I shall say but very little, for the manner of arriving at a positive diagnosis in these cases you are already familiar with.

I will simply remark, in passing, that the first form, the tubercle, is sometimes mistaken for syphilis. In the skin the appearance is very much like the epithelioma of the lip. The third form is very apt to be mistaken for *fungous hæmatodes*. It is perhaps as near to fungous hæmatodes as occurs in the breast. The fourth

form is usually recognized by the pain and retraction of the nipple. The fifth form might raise the question of hydatids of the breast. Hydatids of the breast are exceedingly rare. I have never seen a case. When I find a smooth tumor in the breast filled with fluid, I regard it as being either of the encephaloid or cystic variety of the disease, and the diagnosis between these two is sometimes difficult. The presence or absence of fluctuation and the microscopical evidence are the most reliable.

PROGNOSIS.

First—Prognosis is always unfavorable.

Second—In tumors commencing benign, prognosis is most favorable.

Third—In sloughy tumors, prognosis is more favorable after an operation.

Fourth—In cystic tumors, prognosis is more favorable for an operation, and removal of the tumor is advised.

Fifth—In tumors which commence malignant, the patients usually die within two years. Sometimes they continue a little longer. Yet of tumors commencing malignant I have 14 cases where the patients lived many years after amputation of the breast.

Sixth—Prognosis may be affected somewhat by the method of treatment employed. Hence we have prognosis after compression, electrolysis, caustics and internal treatment, and these will be more particularly spoken of under the head of

TREATMENT.

With regard to treatment, again, I have not much to say. The methods employed may be embraced under the following heads:—

1. Amputation; 2. Caustic applications; 3. Compression;
4. Electrolysis; 5. Medication; 6. Moral treatment.

In the superficial cancer of the breast it is very well to use caustics. The same thing may be said with regard to cancers upon the face. The treatment with caustics in that region is good surgery. When the tumor is situated to any extent below the surface, the idea of caustics is bad surgery.

In two cases which have come under my observation, one died within four days, poisoned by the material used for the caustic application, and the other never reached her home alive.

With regard to the treatment by the use of compression, compressed sponges being usually employed, I have seen no good from it.

With regard to electrolysis, I have seen nothing in it yet to give me any confidence whatever in its use. I have nothing, however, to say against its use. There may be something of value in it, and it should be thoroughly tried. The day was when no knowledge was had with regard to a successful method for the treatment of

syphilis, but now we know that by the proper use of proper remedies, that disease can be cured, and charlatanism has left that field almost entirely. With regard to the internal treatment of cancer, I believe very much in it.

INTERNAL REMEDIES.

I believe that the day must come when something will be accomplished by the aid of internal remedies. Of the remedies now used, arsenic is perhaps the one which commands my confidence more than any other. There is another point in the treatment of cancer which I conceive to be of great importance, and that is, the moral condition of the patient. I believe that it is impossible to cure our patients of cancer unless they are buoyed up by hope. Their surroundings should be of a character that will give them the greatest possible amount of comfort and happiness. Keep them in the sunlight of enjoyment, for darkness is the soil in which cancer flourishes.

THE QUESTION OF OPERATION.

Now we come to consider an important question: Do we accomplish any good by operations?

There are some who say, Never operate. I think this opinion comes from the older members of the profession, who are inclined to look beyond the simple performance of the operation. The younger men, many of them, say, Operate. Upon this question I perhaps can do no better than to refer you to the opinions of two men who are among the most experienced of the profession, and who have had abundant facilities for making observations. I refer to Paget and Sibley, both of London.

Mr. Paget has shown, from his statistics, that the average length of life after an operation is 43 months, and that the average length of life without an operation is 55 months. Mr. Sibley has shown from his statistics that the average length of life after an operation is 53 months, and that the average length of life without an operation is 32½ months. Here are the results of observations made by two distinguished authorities. I think all that I am justified in saying upon this point is, that every case must be taken by itself, looked at with all its surroundings, before a decision is given either for or against an operation.

The dangers in an operation are not great, if it is decided to perform it.

The following may be regarded as the indications, when attempting to decide upon any given case.

The older the patient, other things being equal, the more favorable for operation. If the cancer has extended so that we have secondary cancer, it is not surgical to operate. Therefore, when the axillary glands are involved, or when the skin is involved, and

we have the local and constitutional disease both existing, I regard it as unfavorable for an operation. When the tumor is isolated, and there are no secondary manifestations, the conditions are favorable for an operation, and the sooner it is performed the better is the chance of preserving the life of the patient.

If a patient comes complaining of an irritable tumor of the breast, apparently connected with some disorder of menstruation, I should recommend, first, careful attention to the general health; and second, if found increasing in size, to remove it at once. There is another condition in which I would operate, and that is in the sloughing cases. Then it is done simply to make the patient more comfortable. Practically speaking, these cases do not belong to secondary cancer, and the operations are not unfavorable. But with all these cases we must use our own discretion. Select the cases, and give them the benefits and advantages of an operation.

Now a few words with regard to the hereditary character of cancer. In the cases which are found in my tables, the cancer taint was present in only 28 of the whole number, 236, whose history upon this point was obtained.

Within the last year I have been examining the Register's Bureau of Statistics in this city, and I find, in a period of time extending over about 70 weeks, there were only 532 deaths from cancer of all kinds and in all organs, while from pulmonary consumption alone there were 6,219 deaths, or as 1 to 11½. When compared with Bright's disease it is found that about three times as many die from that disease as from cancer. From the statistics of the Register's office for the last five years, the following proportion of Americans and foreigners who died of cancer are found:—

Americans, 68; foreigners, 154; negroes, 5. Savages rarely have the disease.

It would seem as if this disease of the breast is found in certain conditions of life, and that in these conditions it is upon the increase.

Without pursuing the discussion of this subject farther, I will close by saying, that the conclusions to which I have arrived are chiefly as follows:—

1. That the disease is not hereditary, or if so, only in a very limited degree.

2. That the disease begins as a local disease positively and purely. It becomes constitutional, just as syphilis begins a local disease and becomes constitutional.

3. That the disease occurs in those of vigorous health, instead of being connected with those conditions in which consumption occurs.

4. That cancerous parents may beget tuberculous offspring. That is, feeble constitutions arising from the effects of cancer will not beget cancer, but the diseases which follow in their line are tuberculous.

5. That the moral condition has a powerful influence on the development or the prevention of the development of cancer.

6. I am very forcibly struck by the parallelism and analogy existing between cancer and syphilis. Both begin by local irritation. Syphilis is inoculable, but cancer has not been proven so to be. In this respect they differ from each other. We have secondary syphilis, and we have secondary cancer. We have tertiary syphilis but perhaps it cannot be said that we have tertiary cancer, unless it can be said that cancer is tertiary when it affects the bones, as it sometimes does.

In conclusion, I have to say, that we must not give this subject over as an unprofitable one for study and observation. Many diseases have run rampant which finally have been made to yield to treatment, and we may hope that the same thing may yet be accomplished with reference to cancer. The work of the histologist and pathologist may yet bring us into the light, and the day may come when we can say of cancer, as we now can say of syphilis, It can be cured.

DR. FORDYCE BARKER : Mr. President, my apology for departing from my usual rule with regard to surgical questions and operations is, that I may perhaps suggest some new fields for inquiry and observation, and perhaps bring out some new ideas in the course of the discussion by these suggestions. In regard to surgery, I am no expert. I do not pretend even to interfere with it, and it is therefore somewhat embarrassing to speak upon a subject which really belongs to the surgical department. I have, however, had occasion to study the subject of cancer with great interest, and perhaps with a large experience, and have, therefore, for many years taken every pains to inform myself with regard to the progress of science, and have felt an interest in its bearing upon the question of its manifestation in the form in which it occurs secondarily, which in its most frequent form is in that of cancer of the breast.

In alluding to certain points in connection with the general subject, I will refer to one or two cases in connection with my own personal experience. Previous to my coming to this city, I was obliged to practice more or less in general surgery, and in the course of that time I was called upon to amputate the breast thirteen times, for what I supposed to be cancer of that organ. I have listened to the statistics from the gentleman who has already occupied your attention, with great interest and with great pleasure, because, in almost every point, while they have not corresponded with published statistics as we now have them, they have corresponded with my own. In four of these thirteen cases in which I operated for cancer of the breast, I know nothing of the results. Two of the thirteen cases are still living. All of the seven remain-

He select the following from our discussion
 on Cancer of the Breast at a recent meeting of the
 Medical Library and Journal Association of New York

ing cases died at periods varying from eighteen months to four years after the operation. A curious point in relation to them was, that the one who lived the longest—and this point I have not seen alluded to by any author—was the patient who was the oldest. That patient was 71 years of age when I operated, and had been afflicted with the disease some four or five months when I first saw her.

There was no apparent return of the disease until several months afterwards, and then there was probably a return of the disease to some internal organ. The point is this: whether the progress of pathological changes is not exactly in the same ratio as the metamorphosis of tissue in relation to age; whether in persons of advanced life we may not account in this way for the longer exemption from a fatal termination of the disease than when the disease occurs in those who are less advanced in age.

In 1858, although I had refused to have anything to do with general surgery, and confined my operations entirely to the obstetrical department, I had one patient who absolutely refused to permit any one else to operate upon her except myself. I accordingly removed her breast. The axillary glands were not involved, but the disease returned within a very few months, and the patient died eleven months after the operation.

The second case which I will refer to is a rather curious and rather exceptional one. It occurred in the year 1860, in a lady 43 years of age, and she had had the disease for several months when I first saw her, and in what I regarded as a very malignant form. That person, again, utterly refused to have an operation performed unless I would perform it myself, and I accordingly performed the operation, assisted by Dr. Foster Swift and Dr. Charles Phelps. In that case acupressure was employed, as I believe, for the first time in this city, and I was very much interested and pleased with the effect of acupressure in diminishing the amount of suppuration, which in that case was very slight indeed. That patient was operated upon in April, 1860. In my own belief, and in the belief of the microscopist, it was one of the most malignant forms of this disease of the breast, and yet the woman was alive in 1871. I simply mention this case as a small contribution to the number of successful operations, in the sense of curative, in cases of carcinoma of the breast. That specimen was afterwards presented at the New York Pathological Society, and the minutes of the meeting, which were published in the *Medical Record*, represented it as being presented by Dr. Swift and that the operation had been performed by Dr. Parker, which is a fair illustration of the uncertainty of surgical glory. With regard to statistics in determining whether a surgical operation shall be performed or not, most modern writers agree that operations do, in a certain proportion of cases which are judiciously selected, absolutely and positively

prolong life, relieve suffering, and in some cases actually save life. The diametrical opposition which the statistics of some surgeons have to those of other surgeons who are equally well situated for making observations, may perhaps be explained in this way. One surgeon may be of the opinion that the disease is, primarily, always a local disease, and that its constitutional character is secondary to the local disease, which manifests itself differently in different cases. If this theory be correct, the proper method of treatment is the early extirpation of all suspicious-looking growths. On the other hand, other surgeons are of the opinion that the disease is a constitutional disease; that operations are deleterious in their effects, and should not be resorted to until all other means have failed to arrest its progress.

Again, some surgeons who have a greater fondness for operations than others, will remove a suspicious-looking growth much earlier than those surgeons who are less fond of operations, so that in some cases it may be that the delay in the performance of the operation has permitted the disease to make such extensive ravages upon the general system, that the operation, if performed at all, can only be performed with the expectation of giving some relief from distressing symptoms.

I began in early life as a most enthusiastic believer in the numerical system, regarding it as a most efficient means for advancing our knowledge of disease. But my experience has proven to me that statistics which ordinarily receive publication are extremely unreliable, and that they form a most unstable foundation upon which to predicate future action, whether it shall be for the formation of an opinion or made the basis of an operation. The statistics which the author of the paper has given us relative to the comparative frequency of cancer of the breast singularly accord with the statistics taken from the cancer hospitals in the city of London. Out of 7,800 cases which were under treatment in that city between the years 1851 and 1861, 4,388 were cancer of the breast. This is from an entirely different sphere of observation, and yet the result of the observation shows that the female breast is one of the most favorite places in the human body for the development of this disease. It seems to have an elective affinity for the female breast, and perhaps in the progress of etiology and the science of physiology the reason for this elective affinity will be discovered.

The next point which I will notice in connection with the paper, is with regard to hereditary predisposition to the disease. I feel quite confident that I should never have read a paper which I did read, and which was published by the Academy of Medicine, upon "The Clinical Study of Cancer of the Uterus," had I not been thoroughly convinced upon this point. When I came to study my own observations, I found that some of them were so different

from the published statements in published works that I felt doubtful about reading them without consultation with some of my personal friends. My own statistics with regard to hereditary predisposition to cancer of the uterus almost exactly correspond to the observations of the author of this paper with regard to hereditary predisposition to cancer of the female breast.

Another very interesting point to me was, that the author of the paper has found so much larger proportion of cases of cancer of the breast where hereditary predisposition to cancer was entirely absent, but where hereditary predisposition to tubercles was present. The results of his observations upon this point give the same relations which are found in my own statistics, and I believe that the idea of hereditary predisposition to cancer should be denounced, and that this denunciation should be pronounced boldly by physicians.

There were a few points to which no allusions were made, and concerning which I wish to make some inquiry.

What is meant by a cancerous cachexia? In my earlier experience I was always looking for something like a cancerous cachexia, but my later experience and observation have taught me to become a non-believer, and I do not now believe at all in cancerous cachexia, as the term is commonly used. I have seen patients in the most advanced stages of cancer of the uterus, and in almost all its various phases, when they presented the appearance of robust health. The cachexia, when it does appear, is to my mind not a measure of the influence which has been produced by the simple presence of cancer in the system, but rather from associated lesions of the various organs of the body.

These are my observations with regard to cancer of the uterus, and I should like to know whether the same thing has been observed with regard to cancer of the breast.

Another point, which was not alluded to, and concerning which I should be pleased to gain some information, is, with regard to the value of pain as a symptom in cancer. I am of the opinion that it is a symptom of uncertain value in aiding us in determining the existence or non-existence of cancer of the uterus. I have seen patients in the advanced stages of the disease without the slightest suspicion having been raised with reference to the presence of the disease by any pain. My own opinion is, that pain is simply a measure of the influence which the disease has had upon the contiguous and adjacent tissues. Cancer may occur so as to interfere with the functions of the uterus, or affect the sub-peritoneal tissues; and when these tissues are affected we are sure to have pain, and in some of these cases the pain is most atrocious. In other cases, where the disease presents more malignancy, the pain is sometimes very trivial. Whether the amount of pain is in relation to the amount of influence which the disease has upon the

adjacent and contiguous tissues, I am not able to say, but simply throw it out as a question for consideration.

From time immemorial there has been an attempt made to destroy cancer by the use of every variety of known caustics. It has been a favorite resort of empiricism, and the most successful and perhaps the most lucrative of all charlatanism has been seen in the use of caustic agents to destroy the local manifestations of cancer. As a consequence of this, of course, a great majority of the surgical world have been satisfied with regard to the uselessness of such attempts. My own prejudices have always been against this method of treatment. I once attempted to make some observations respecting this plan of treatment as it was then adopted in St. Bartholomew's Hospital, and the whole process was so revolting that I did not pursue my investigations farther, and the result of my observations was not at all favorable.

In the year 1870, however, I was consulted by a lady who had a tumor in the breast which was very suspicious in its character, and which I watched for some weeks, when I regarded it as cancer, and urged upon my patient the importance of having it removed at once. But that patient utterly refused to have any cutting operation performed. At that time I had been studying up the subject somewhat, and among other works which I had read was Marsden's work upon the use of caustics in the treatment of cancer.

The same summer, while abroad, I visited the hospital in which Dr. Marsden had made his observations and applied his treatment, and saw the results of this treatment. I became so much interested in this plan of treatment and was so highly pleased with it, that, upon my return, I recommended to my patient to submit to the treatment by the use of caustics. After some delay she consented. The form of cancer from which she was suffering was apparently of the most malignant type, and at the time I commenced the treatment the mass was about two inches in diameter, which is the extreme limit in size permissible to be treated by this method. In the course of eighteen days after the first application was made, the mass came away, the process of cicatrization was completed in a short time, and there has not been the slightest appearance of return up to this time.

Another case to which I wish to make reference, was in a patient who had had two sisters die with cancer of the breast, but her father and mother were still living at the time she consulted me. Not the slightest suspicion of cancer could be traced in either member of the family. One sister died some six or seven years ago from cancer of the breast. The other sister I was called to visit, and I found the axillary glands involved in the disease; there were evidences of what is known as the cancerous cachexia, and I called for council. Dr. Van Buren was called in consultation, but the case was regarded as utterly hopeless, and the patient died without an operation.

The third sister came under my observation for epithelioma of the uterus. That patient I operated upon in 1866, removing the cervix uteri by amputation. It is now seven years since the operation was performed, and she remains in the most perfect health.

About five years ago a lady consulted me with regard to a suspicious-looking tumor in her right breast. She was under my observation for about two years, and received treatment, but I never was of the opinion that the growth was malignant. At the end of two years it entirely disappeared. In February, 1873, that patient came back to me with a tumor in her left breast, which I regarded as true cancer of the breast. The tumor had been observed for more than a year, and when I saw it, the nature of the case seemed clear and positive. Its removal was recommended. Consultation was held, to satisfy the patient with regard to its nature, the propriety of its removal, and if decided to remove it, how it should be removed. It was decided to remove the tumor by Marsden's treatment, and the treatment was accordingly commenced upon the first day of April. The amount of pain which the patient has suffered during the course of the treatment has been very insignificant indeed. She has been up most of the time, has been able to be out riding some of the time, and it is now eighteen days since the first application, and the slough is just ready to come away. The treatment of this case thus far has been very pleasant. What the result of the case may be it is impossible at present to decide.

I will now describe the plan of treatment as given by Dr. Marsden—the plan which he professes to have derived great success from, not only in a very considerable number of cases of cancer of the breast, but in the treatment of cancer of various parts of the body, and even of cancer of the neck of the uterus.

This method of treatment is limited to cases in which the surface of the tumor does not extend over two inches. Care must be taken that the paste is of sufficient consistence so as not to flow beyond the point to which it is applied. The general formula for the preparation of the caustic is to combine arsenious acid and mucilage in such quantities as to make a thick paste, and the formula commonly employed for this purpose is—

R.	Arsenious Acid,	-	-	-	-	-	-	-	dr. ij.
	Mucilage,	-	-	-	-	-	-	-	dr. j.

This paste is spread over the surface of the tumor, and two or three layers of lint spread over that. The lint absorbs all the surplus paste and protects from farther cauterization. The first application is left on for twenty-four or forty-eight hours, according to the extent of surface, and then removed by gently soaking it with warm water. After the old paste has been removed in this way, one judges from the impression made with regard to a farther

application of the caustic. These applications are to be continued until a line of demarcation entirely surrounding the diseased structure is shown. Then the lint is soaked and removed, and a bread-and-water poultice applied, and changed every few hours. At first there is sometimes considerable inflammatory action set up, but the amount of pain is very inconsiderable as compared with the use of the knife, and the process of cicatrization is equally painless and satisfactory.

The shock to the system, as a rule, is very much less. The constitutional effect of the arsenic in this case was very slight, lasting only a few hours, and then passed away. Indeed, the moderate constitutional effect of arsenic I have long believed to have a certain positiveness in the treatment of cancer, in that it retards the proliferation of cancerous tissue. I mention these cases with the hope that it may contribute something to our knowledge of means by which we may meet this most terrific disease. //

Med Record

DR. SANDS: Mr. President, I shall merely call the attention of the Association to a few points in connection with the nature of cancer, the classification of cancer, the etiology of cancer, and the treatment of cancer. I am more and more convinced, until surgeons can have the aid of the pathologists, until they can work together in the investigation of malignant growths, that but little advance can be made with regard to the important subject of the therapeutics of cancer.

It is a fact with which we are all familiar, that at the present time various kinds of tumors are removed from the body, and the most varying opinions are expressed with regard to these cases. It is not long since that I removed a female breast, and one surgeon who was present expressed the opinion that it did not need any microscope to enable him to determine that it was cancerous; but a microscopical examination proved it to be a case of pure adenoma of the breast. I think that most surgeons, as well as pathologists, are willing to give an anatomical rather than clinical meaning to the term cancer. The term cancer can no longer be considered synonymous with malignancy. All cancerous tumors are malignant, it is true; but, on the other hand, there are many tumors which are malignant in their nature, to which the term cancer cannot be applied, because they have not the anatomy of the tumors described under that term. Among these we have the sarcomata, myxomata, epitheliomata, lymphomata, and some others. If we are to make any progress in our investigation of this subject of tumors, each tumor should be examined immediately, with the view of determining the true anatomical elements of which it is made up.

With regard to the etiology of cancer, of course it is very interesting and important to determine whether cancer is, or is

not, a local disease, whether the condition of the patient is always consequent upon a constitutional taint, or whether it is simply a symptom incident to a local morbid state. Different views have already been given upon this point; and in favor of its being a local disease primarily, there is a degree of plausibility, when we go a certain distance. The occurrence of cancer secondarily can be explained in this way, when it occurs in or near the original seat of the disease. But it does not so easily explain the occurrence of cancer in parts *remote* from the original seat of the disease. In illustration of this point is the following case:

I had a patient under observation who was suffering from primary melanotic cancer of the lymphatic glands in the axilla. The diseased glands were removed, and the patient had a good recovery, but afterwards suffered from secondary cancer of the scalp. In the latter situation the disease was identical with that of the lymphatic glands in the axilla.

Another case illustrating the same point occurred in a person from whom I removed, about eighteen months ago, a small epithelioma of the face. Dr. Parker had removed a similiar growth from the same situation before. This gentleman made a good recovery, but he is now suffering from epithelioma of the rectum, and is rapidly declining in health. I have still another case, where it is yet more difficult to explain the secondary occurrence of the disease upon the theory that cancerous tumors are transplanted. That case was seen in St. Luke's Hospital, and was a small tumor which occurred in the left ventricle of the larynx. The tumor was removed, and the patient made a good recovery. The disease was never reproduced in the original locality. The nature of the tumor was determined by microscopical examination to be cancerous. About eighteen months subsequent to the operation, the patient again entered the same hospital, and died twenty-two months after the original operation. When post-mortem was made, cancerous disease was found in the suprarenal capsules of both sides and one ureter. It would seem hardly possible to explain this case upon the theory that secondary cancers are always the result of primary cancers; and all these cases which I have mentioned go very much to show that cancer is a constitutional and not a local disease. Some surgeons go so far as to say that all tumors are expressions of constitutional disease. It is a well-known fact that, in some cases, benign tumors are so numerous that their occurrence in such numbers can only be explained upon the theory that they are dependent upon some constitutional cause.

With regard to hereditary tendency of cancer, Dr. Parker's statistics are very gratifying; but there are some facts which I think show that in some cases, at least, cancer can be inherited. Cases like the following would seem to indicate that the disease has a hereditary tendency. In that case autopsy demonstrated that the

gentleman died from cancer of the stomach, and not long afterwards a son of the same gentleman died with cancer of the same organ. I have already in mind several cases equally well marked, which it seems to me clearly indicate that the disease has a hereditary tendency. With regard to the treatment of the disease, I have had no experience with reference to the use of caustics. I have, however, a very decided conviction that the knife is the remedy, so far as there is a remedy in this disease. I do not believe that cancer is purely a local disease, yet if the knife can be applied early, and remove not only all the tissue of the disease, but the tissues about the disease, there may be a chance for a long interval before a return of the disease, in some cases.

The reasons why I would advocate the treatment of cancer by operation with the knife are chiefly threefold. One is, that the nature of the tumor cannot be determined prior to its removal. Another reason is, that it now and then happens that a tumor of undoubted malignancy is cured by this surgical operation. Finally, I think an operation is justifiable often upon the ground of expediency. The chances are increased, if the patients die from cancerous disease, that they will die not from the external progress of the cancer, but that they will die from cancer of some of the internal organs. It is very well known that the return of the disease is very often upon the internal organs after the external disease has been removed, especially upon the lungs, and a death from cancer of the internal organs is not so painful and not absolutely loathsome, as when permitted to pursue its ordinary course externally.

DR. BUCK related the history of a case of cancer cured in the person of a lady about 40 years of age. The tumor was in the breast, and possessed all the characteristics of scirrhus. It was removed, and more than 30 years after the operation it was ascertained that the patient was yet living and in comfortable health.

Another creditable case was in a lady who was very timid and despondent. One determining point in this case was, to perform an operation in order to infuse hope into the mind of the patient with regard to her recovery. She did make a favorable recovery, and subsequently returned to France. That lady was well for two years, and I was fully convinced that she would not have survived as long as that without the operation. In these days of anæsthetics we may more readily advise an operation, for the patients are entitled to the respite of hope and comparative comfort which may be more than an equivalent for the fears and sufferings which they may experience without the operation.

With regard to operating for the removal of cancerous breasts in very corpulent persons, my experience has been very discouraging. I always dislike to perform an operation upon such patients, for in

some cases which I have seen their life is drained away, as it were, from the wound.

DR. THOMPSON: Mr. President, upon perusing the records of the New York Pathological Society, from 1845 to the present time, I find that 55 cases of carcinoma of the stomach have been reported by various surgeons, of which Dr. Parker reported four. The first case was reported by the President of this Association, Dec. 24, 1845.

Of those 55 cases, 23 were males and 18 females.

In fourteen cases the age of the patient was not reported. The average age of the cases reported was $49\frac{1}{2}$ years, which is a little above the average age of Dr. Parker's cases, which he reports at $48\frac{1}{10}$ years.

Remarks by the President.—Gentlemen and Associates: We may well congratulate ourselves upon the results of this evening's investigations and discussion. We have become better acquainted with the real nature of cancer of the breast, and with those hygienic, therapeutic and operative means which will not only delay its progress and mitigate its dire effects, but which may in a certain number of cases remove it entirely.

The time has now arrived when its treatment is removed from all empiricism, and the regular profession is in possession of means which will accomplish vastly more for the sufferer than any boasted specifics, or any irregular or mysterious practice.

We now know that cancer of the breast is generally a simple and local, not a constitutional or hereditary affection. It is a mere excessive growth and proliferation of normal epithelium. The first change is a large accumulation of epithelium within the ducts of the gland, just as in ordinary adenoma. But soon the walls of the ducts are not only invaded by epithelium, but this is also found outside the ducts amongst the inter- and peri-glandular connective tissues, which in their turn are thrown into a state of active proliferation, and become infiltrated with small round cells. Now, we have every reason to believe, from the vast experience of Weeden Cooke, that the muriate tincture of iron and other mineral astringents will arrest these processes in their incipency, and much delay them in their advanced stages.

We also know that the epithelial cancer-cells are not cemented together by any intercellular substance, and that the lymphatics communicate with the alveoli in which the loose and detached cells are contained. The mineral astringents will prevent the absorption of the infecting cells into the lymphatics, and the further contamination of the system.

If the disease be farther advanced, we have every reason to believe that Marsden's and Cooke's pastes will accomplish more than any caustic vaunted by quacks.

Finally, with proper precautions, it has been proved that the knife may often be used very successfully. There is an active and passive stage of cancer; and when the tumor is removed in its active condition the constitution resents this premature interference, and the disease returns rapidly after the operation. The local soothing measures recommended by Weeden Cooke should be used until the tumor has ceased to grow, and then exsection may be performed with safety.—*New York Med. Record.*

Notes on the Pneumogastric. By J. F. ALLEYNE ADAMS, M.D.,
Pittsfield. Read before the Massachusetts Medical Society,
June 3, 1873.

In presenting to the Society a brief paper upon the functions of the pneumogastric nerve, and their bearing upon the etiology and treatment of certain forms of disease, a few remarks will be offered, by way of introduction, upon the anatomical relations of the pneumogastric to the other cranial nerves.

By the classification of Soemmering, the pneumogastriCS constitute the *tenth* pair, and by that of Willis a portion of the *eighth*. The more scientific classification of Muller calls the pneumogastric, with the glosso-pharyngeal and spinal accessory, the *second* pair of cranial nerves, the third, fourth, fifth and sixth of Willis, with the portio dura of the seventh, being grouped together to form the first, while the hypoglossal is the third. This arrangement is more in accordance with the teachings of philosophical anatomy, inasmuch as it harmonizes the nerves with the vertebrate type of the skull, giving one pair for each of the three cranial segments. It leaves out of the question the three nerves of special sense, viz., olfactory, optic and auditory, which are demonstrated by Oken to have no true nerve structure, but to be mere outgrowths or appendages of the brain. But Muller's classification does not complete the homology between the cranial and spinal nerves, for the third pair has no sensitive root, and the distribution of both the second and third is greatly at variance with the anatomical relations of the vertebræ to which they respectively belong.

Dalton* proposes a fourth classification, which is identical with Muller's, excepting that the hypoglossal is put with the glosso-pharyngeal to form the second, while the pneumogastric and spinal accessory constitute the third. Although suggested by him on purely physiological grounds, this arrangement commends itself, as well, from an anatomical point of view; and there are strong arguments in favor of its adoption as the true classification of the cranial nerves.

* Dalton's Human Physiology, p. 432.

These arguments may, for convenience, be arranged under the three following heads:

1st, Origin; 2nd, Distribution; 3rd, Function.

1st. *Origin.* All of the cranial nerves, excepting the three nerves of special sense mentioned above, have their deep origin in the medulla oblongata, and are arranged in the following order: from its upper portion arise, in close proximity, the trigeminus, the facial, motor oculi, abducens and patheticus; then comes the glosso-pharyngeal, next below which is the deep origin of the hypoglossus; below this is the vagus, and, lastly, the accessorius. Thus we see that the hypoglossus, although having its origin on the same level as the vagus, and anterior to it, really rises above that nerve, and close to the origin of the glosso-pharyngeus. Arguing from the deep origin, therefore, there is no propriety in calling the hypoglossus the last of the cranial nerves.

2nd. *Distribution.* The several nerves grouped together as the first pair are distributed chiefly to the frontal segment, including its inferior or hæmal arch, the inferior maxilla. The second, or parietal segment, has for its hæmal arch the styloid processes and hyoid bone; and we find the glosso-pharyngeus and hypoglossus distributed to the tongue and pharynx, parts intimately connected with these bones. The third, or occipital segment, by an adaptation to special ends, has its hæmal arch separated from it, to form the scapular arch; and, accordingly, the third pair of nerves undergoes a similar deviation, the pneumogastric and accessorius being distributed to the whole respiratory apparatus, the heart and chylo-poetic viscera, while the spinal portion of the spinal accessory goes to the trapezius and sterno-mastoid muscles, which form the chief connecting links between the occipital vertebra and its hæmal arch.

3rd. *Function.* The present classification makes the analogy of the cranial to the spinal nerves the more striking, in that it gives to each pair a sensitive and a motor root. Thus, the first pair has for its sensitive root the trigeminus, and for its motor the facial, of which the three motor nerves of the eye and the motor root of the fifth pair are mere accessories. The second pair has the glosso-pharyngeal for its sensitive, and the hypoglossus for its motor root, while the sensitive root of the third pair is the vagus, and the motor the spinal accessory.

Of the arguments that may be adduced in antagonism to this classification, the strongest is, that, in frogs, the hypoglossus is placed *below* the vagus, and becomes the first of the spinal nerves; although the deep origins of the two nerves, even in this case, have been shown by Vogt to be close together.* Another objection is, that the hypoglossus leaves the skull by the anterior condyloid foramen, which is posterior to the foramen lacerum posterius,

* J. Wyman. Nervous System of Rana Pipiens. Washington, 1853.

which transmits the pneumogastric. Therefore, although both of these objections might possibly be ruled out, the arrangement is not to be accepted without due caution.

The pneumogastric arises, superficially, by eight or ten filaments, from the lateral portion of the medulla oblongata, behind the olivary body, between the roots of the glosso-pharyngeal and accessorius. Its deep origin is from a grey nucleus in the lower portion of the floor of the fourth ventricle. The nerve leaves the cranial cavity by the foramen lacerum posterius, within which is formed the ganglion of the root, the ganglion of the trunk being external to the cranium. The root of the pneumogastric is strictly sensitive, but its trunk is both sensitive and motor. Its motor filaments are derived chiefly from the accessorius, which unites with it just below the ganglion of the trunk, besides giving off some filaments to the ganglion of the root. It likewise anastomoses at the ganglion of the root with the facial and hypoglossal, and, at that of the trunk, with the hypoglossal and first two cervical nerves. Both ganglia are intimately connected by numerous filaments with the superior cervical ganglia of the sympathetic, while its trunk receives branches from the middle and inferior cervical and upper dorsal ganglia.

Thus constituted and reinforced at its origin, the par vagum is prepared for a distribution more extensive and functions more varied and remarkable than any other pair of nerves. Its functions may be briefly stated as follows:—Its *sensitive* filaments are distributed, through the auricular, to the external auditory meatus and membrana tympani, through the superior and inferior laryngeal, anterior and posterior pulmonary, to the whole respiratory tract, through the cervical and thoracic cardiac to the heart, and through the pharyngeal, œsophageal and abdominal branches to the digestive tract.

Its *motor* filaments are distributed, through the pharyngeal and inferior laryngeal branches to the pharynx, through the recurrent laryngeal to all of the muscles of the larynx except the crico-thyroid, through the superior laryngeal to the crico-thyroid, through the recurrent laryngeal and pulmonary branches to the tracheal and bronchial muscular coats; and, through the œsophageal, gastric and intestinal branches to the muscular coats of the œsophagus, stomach, and the whole of the small intestine. Until the discovery of the intestinal branches by Kollman, in 1860, the abdominal distribution of the vagus was supposed to stop at the pylorus.

The pneumogastric sends *secretory* fibres to the gastric and intestinal mucous membrane.

Its hepatic branches preside over the glycogenic function of the liver.

It also sends inhibitory or arresting fibres to the heart.

It transmits accelerating and inhibiting fibres to the centre of respiration.

It sends accelerating and inhibitory fibres to the vaso-motor centre, the latter being contained in the depressor nerve of Cym and Ludwig.

Without attempting to go over all of the ground here indicated, this paper will confine itself to the more recent discoveries concerning the relations of the pneumogastric to the heart, the lungs, and the stomach.

Influence of the Pneumogastric upon the Heart.—Although the vagus supplies to the heart both centripetal and centrifugal filaments, its action upon this organ is not that of an ordinary motor nerve. On the contrary, it exerts an arresting or regulating influence. The rhythmical contractions of the heart are induced by an excitation derived from the cardiac ganglia of the sympathetic system, beautifully described and figured by Pettigrew, in the May number of the *Edinburgh Medical Journal* for the present year.* This system bears to the heart the same relation that the main-spring does to a watch, while the pneumogastric answers, in some sort, to the escapement. This influence is commonly known as the *inhibitory* action of the pneumogastric.

The subject of inhibition was first introduced to the profession in 1846, by the brothers Weber, who discovered that galvanization of the pneumogastrics in the neck rendered the action of the heart slow; and if the current were sufficiently powerful, arrested the heart in diastole. Since then, numerous experiments have demonstrated the accuracy of their observations, and have added largely to our knowledge in this direction. The result of these experiments may be stated as follows: †

If one of the pneumogastrics be divided in the neck, the frequency of the heart's pulsations is slightly increased and the cardiac pressure slightly diminished.

If both pneumogastrics are divided, the frequency of the pulsations is greatly increased, at least doubled, while the force is greatly diminished.

Galvanization of the peripheral end of the divided nerve retards or arrests the heart's action; but galvanization of the central ends produces no such effect, showing that the inhibitory influence is a centrifugal one.

If an animal be placed under the influence of woorara poison, which paralyzes motor nerves without affecting sensitive nerves or muscular irritability, and the pulsations of the heart be kept up by artificial respiration, galvanization of the pneumogastrics has no

* James Bell Pettigrew. On the Physiology of the Circulation. *Edinburgh Medical Journal*, January to May, 1873.

† Flint. The Physiology of Man. Vol. IV. Nervous System. 1872.

effect upon the heart's pulsations; whence we may conclude that the inhibitory power is located in motor filaments. This experiment has been tried by Bernard and verified by Flint.

The experiment has been performed by Traube of injecting digitalis into the veins of a dog with the pneumogastrics divided, without affecting the heart's pulsations; although, when this was done with the nerves intact, the number of beats was, in an hour, reduced to one-fourth of the normal number. This indicates that the sedative action of digitalis upon the heart is due to an irritation of the vagus, transmitted from the brain.

Irritation of the medulla oblongata produces the inhibitory action; but the irritation is conveyed, not by the root of the vagus, but, as has been demonstrated by Waller, by the accessorius.

Recently, it has been discovered that the right vagus possesses a greater inhibitory power than the left. "Dr. Massin states that, while the movements of the heart are stopped by a very powerful excitation of the left vagus, it is always possible, by sharply striking the heart with the finger, to reproduce the rhythmic movements; while this mechanical cause of action remains ineffectual when applied to the heart stopped by strong galvanization of the right vagus."^{*}

The inhibitory influence upon the heart is also exerted in various reflex ways. Brown-Sequard[†] has found, in rabbits and guinea pigs, that an irritation of the nostrils by carbonic acid or chloroform, or even by a sudden and gentle touch, will cause the heart to stop completely, or, at least, notably diminish its force and speed. Rutherford states that, if any irritating vapor be brought before the nose of a rabbit, it closes its nostrils and ceases to breathe, often for thirty or forty seconds. Within three seconds after the cessation of respiration, the heart comes almost to a stand-still, and continues to beat very slowly until respiration is re-established. Rutherford believes that "this arrest of the heart is due to stimulation of the inferior cardiac branch of the vagus by the asphyxiated condition of the blood;" but Brown-Sequard proves conclusively that this is not the case, but that the phenomenon is due to reflex influence.

The cold bath, especially the shower baths or douche, has, by reflex action, the same inhibitory influence upon the heart. Fainting is even produced in some persons by this means, from complete arrest of the pulsations. Brown-Sequard states that, in Europe, where ice-water is not as habitually used as here, a stoppage of the heart's action is not rare, in the summer season, after the drinking of a large quantity of cold water.

^{*} Brown-Sequard. On the Sudden or Rapid Arrest of Certain Normal or Morbid Phenomena. Archives of Scientific and Practical Med., Jan., 1873.

[†] Ibid. Most of the physiological facts in this paper are from Flint or Brown-Sequard.

Wounds of the abdominal viscera and blows upon the belly produce fainting or slowing of the heart's action. To Brown-Sequard we are indebted for the discovery that the crushing of the ganglions of the sympathetic in the abdomen stops or diminishes the movements of the heart; but if the spinal cord or vagus is divided transversely, the crushing can be made without any influence upon the heart.

Goltz has demonstrated, in the frog, that the heart can be brought to a stand-still by striking upon the abdomen, and that this paralysis of the heart can be prevented by a simultaneous strong irritation of the cutaneous nerves, which diminishes or abolishes the reflex activity.

It is asserted by Rouget, that the inhibitive action takes place in nerve-cells. He says: "Wherever the excitation of a nerve is followed by an arrest of movement, nerve-cells are found on the nerve-fibres which transmit the excitation." In this view he is supported by Brown-Sequard, who thus describes the process of arrest:

1. An irritation starts from a part of the nervous system radiating towards the nerve-cells, the activity of which is to be arrested.
2. A special influence is to be exerted by this excitation on those cells, arresting their activity.
3. The movements, normal or morbid, or other phenomena depending on the activity of those cells, cease at once when that activity ceases.

In the discovery of the *depressor* nerve by Cym and Ludwig, who first published an account of it in 1867, an important addition is made to our knowledge of the pneumogastric in its relation to the heart. This nerve was found in the rabbit, arising by two roots, one from the trunk of the pneumogastric, and the other from its superior laryngeal branch, passing down the neck, by the side of the sympathetic, to the chest, where, joining with sympathetic filaments, it passes to the heart by little branches between the origin of the aorta and the pulmonary artery. This nerve has since been found by Cym in the horse. In the rabbit it has been made the subject of a very complete and satisfactory series of experiments.

When this nerve is divided in the neck, and its *peripheral* end irritated by galvanization, no effect whatever is produced upon the heart or the circulation; but if its *central* end is galvanized, the pressure is diminished in the whole arterial system. This diminution is gradual, the pressure never reaching its minimum before fifteen pulsations of the heart, and may be reduced to one-half the pressure before the irritation was applied. At the same time that the arterial pressure is diminished, the frequency of the heart's pulsations is reduced, *unless both pneumogastrics have previously been*

divided, when, although the arterial pressure is reduced as before, the action of the heart is not disturbed. These experiments prove that the action of the depressor is entirely centripetal, that its inhibitory action upon the heart is reflex, through the nerve-centres, the trunk of the pneumogastric, and its remaining cardiac branches, while the reduction in the pressure of blood in the large arteries is independent of the centrifugal fibres of the vagus, and bears no relation to the reduction in the number of cardiac pulsations.

In determining the mechanism whereby the reduction in the arterial pressure is effected, Cym has displayed great ingenuity. Having, by a process of exclusion, eliminated all other possible agencies, he has clearly shown that this influence is exerted by reflex action, through the instrumentality of the splanchnic nerves, the most important vaso-motor nerves in the entire organism. "The irritation of the depressor nerve, after section of the splanchnic nerve," to use the language of Cym, "produced still a diminution in the blood-pressure, but the absolute value of this diminution is much less than it was during the irritation of the depressor nerve, before the section of the splanchnic."

The result of these experiments is to show that irritation of the depressor nerves exerts, through the splanchnic, an inhibitory or paralyzing influence upon the abdominal vaso-motor system; and thus, the resistance to the flow of blood being diminished in the immense vascular system of the abdominal organs, the pressure is correspondingly reduced in the arterial trunks.

Influence of the Pneumogastric upon the Lungs.—Recent experiments have made important additions to our knowledge of the nervous processes concerned in respiration. In the first place, Flint conclusively shows that the sense of want of air (*besoin de respirer*) is not communicated to the nerve centres by the pneumogastric; for respiration continues after section of those nerves. Farther than this, Brown-Sequard maintains that the generally accepted view that the centre of respiration is located in the medulla oblongata, at the *neud vital*, or vital point of Fleurens, is erroneous; for, although respiration is instantly arrested by section or a mere prick at this point, yet section of the medulla or the spinal cord below this point, and above the level of the respiratory nerves, does not, in young mammals and adult birds, immediately stop the rhythmic respiratory movements; and he has even found the contractions of the diaphragm to continue for a time, with great regularity, when its communication, not only with the medulla, but also with the spinal cord, has been cut away. From these observations, it is necessary to believe that the diaphragm is, to a certain extent, an independent organ, containing in itself, as does the heart, sympathetic nerve centres controlling its movements. But the vagus, although not indispensable to respiration, yet exerts upon it a

powerful influence, whose nature will best be seen by noting the result of certain experiments.

If both pneumogastrics are divided in the neck, in dogs, the number of respiratory acts is at first increased; but, as soon as the animal becomes tranquil, the number is diminished, sometimes falling from sixteen or eighteen to four per minute. The inspirations are, at the same time, unusually profound, attended with excessive dilatation of the thorax. Death usually occurs in from two to five days, and, in most of the animals so dying, the lungs are found engorged with blood, so that they sink in water. This carnification is explained by Bernard as due to a rupture of the pulmonary capillaries, consequent upon a traumatic emphysema, resulting from the excessively deep and labored inspirations.

If galvanization be applied to the pneumogastric in the neck, the effect upon respiration varies, according as the excitation is feeble or energetic. A feeble current accelerates respiration, and renders it more vigorous, especially in inspiration, although, if the feeble current is applied very high up, near the origin of the nerve, respiration is arrested. If a powerful galvanic current is applied to the pneumogastric, in any part of the neck, respiration is arrested by a spasm of the diaphragm; but it speedily returns, even during the excitation. After the removal of the excitation, breathing is accelerated.

Galvanization of the superior laryngeal nerve, whether energetic or feeble, arrests respiration by paralyzing the diaphragm. Galvanization of the recurrent laryngeal generally produces the same effect. Respiration is also sometimes arrested by the galvanic current applied to the pharynx or œsophagus.

Galvanization of the lungs produces persistent spasm of the diaphragm, and consequent arrest of breathing in inspiration.

From these experiments, it appears that the pneumogastrics exert a normal stimulant influence upon the function of respiration, and that, under certain circumstances, they may also exert an inhibitory power. The inhibitory effect is produced in one of two ways:—first, by paralyzation of the inspiratory muscles, induced by irritation of the superior or inferior laryngeal, the pharyngeal or œsophageal branches; and, secondly, by a spasm of these muscles, from irritation of the trunk of the vagus, or its ramifications in the lungs.

The first of these modes of inhibition occurs normally in the act of deglutition. It is impossible to breathe, while swallowing; not only because the larynx is closed by the epiglottis, but also because there is a momentary reflex paralysis of the diaphragm, from irritation of the pharyngeal and superior laryngeal nerves.

In the operation of tracheotomy, a temporary stoppage of respiration not unfrequently occurs, from irritation of the inferior laryngeal nerve. The same effect has been observed by Brown-

Sequard, on suddenly stopping the mouth of a tube introduced into the trachea of dogs or rabbits, all respiratory effort at once ceasing.

Arrest of respiration also takes place upon submersion. That this arrest is not dependent upon the spasmodic closure of the glottis, which also occurs, has been shown by Bean and Brown-Sequard, by submerging an animal in whose trachea a tube had been placed, whose free extremity communicated with the atmosphere. Even in this case, no effort was made to breathe, or if so, not till after some seconds. Bean asks if this arrest is due to the influence of *cold* upon the mouth and nostrils. Brown-Sequard answers the question by submerging an animal in *warm* water, in a similar manner, and finds respiration again suspended. He concludes that the irritation produced by the contact of either cold or warm water upon the mouth and nostrils, produces, by its *suddenness*, a reflex arrest of breathing. If the trunk or limbs are dipped in water, breathing is accelerated; but it is arrested by the sudden dashing of cold water upon any part of the body—an illustration of Bert's law that any gentle irritation of centripetal nerves increases the number of the respiratory movements, and any powerful excitation diminishes them.

The engorgement of the lungs following section of the pneumogastric in the lower animals, is also seen in man, when, from any cause, that nerve is severed. A case occurred at the Boston City Hospital,* in 1865, of a woman, whose husband very skillfully divided with a knife the right pneumogastric, as well as the internal jugular vein and the neighboring muscles. She was attacked, on the tenth day, with congestion of the right lung, which passed off on the thirteenth.

Coughing is a reflex, spasmodic contraction of the expiratory muscles, produced by irritation of the inferior laryngeal or pulmonary branches of the pneumogastric. In asthma, there is, according to Salter, a spasmodic contraction of the bronchial tubes, caused by pneumogastric irritation. This irritation is generally reflex, from impressions upon the respiratory or gastric mucous membrane, or the skin; but may arise, as well, from cerebral disturbance. Neffel† has contributed some interesting observations upon the treatment of this most obstinate affection by galvanization of the pneumogastric. He uses the polar method, applying a current of gradually increasing intensity, all large fluctuations being carefully avoided, by means of the rheostat, and reports a marked degree of success, the paroxysm usually passing off in from two to ten minutes. But he finds the relief afforded sometimes by the anelectrotonic, and at other times by the catelec-

* Boston City Hospital, Medical and Surgical Reports. 1870. Surgical Report by Dr. D. W. Cheever.

† W. B. Neffel. Galvano-Therapeutics. New York. 1871.

trotonic condition of the vagus. Reasoning from the spasm-theory of asthma, it was to be expected that the former alone could give relief; but Nestel has found that in certain cases the attack is arrested by the latter condition, after the former has failed. In explanation of this peculiarity, he suggests that the theory, held by Walshe and others, that asthma is due to a paralytic condition of the muscular coats of the bronchial tubes, may be true of some cases, while others are the result of spasm, the former class being relieved by catelectrotonus and the latter by anelectrotonus. The idea is certainly plausible, but, in the present inchoate condition of galvano-therapeutics, we must await further evidence before accepting it.

(CONCLUDED IN NOVEMBER NO.)

Editors' Book Table.

[NOTE. — All works reviewed in the columns of the CHICAGO MEDICAL JOURNAL may be found in the extensive stock of W. B. KEEN, COOKE & Co., whose catalogue of Medical Books will be sent to any address upon request.]

BOOKS RECEIVED.

Report of the Columbia Hospital for Women and Lying-in Asylum, Washington, D. C. By J. HARRY THOMPSON, A.M., M.D., Surgeon-in-Chief. With an Appendix. Washington: Government Printing Office. 1873.

One of the most valuable contributions to gynæcological medicine extant, comprising the results of the treatment of over eleven thousand cases of various phases of the maladies incident to womanhood, together with copious appendices, consisting of reports from the departments of the Diseases of Children, and of Diseases of the Eye and Ear.

The establishment and maintenance of the Hospital whose "Report" is before us, is a gratifying evidence of the appreciation of our representatives in Congress of their duties in regard to the care of residents of every portion of the country, and indeed of the world, who, having been attracted to Washington by their business relations to the General Government, and being prostrated

by disease would have become a tax upon the citizens of the District of Columbia, but for this most humane and enlightened provision for their necessities.

The list of beneficiaries of this national charity comprises residents of twenty-four States and twelve foreign countries, a diversity of origin and constitutional peculiarities complicating morbid conditions, but at the same time adding value to the result. The surgeon-in-chief appears to have been peculiarly successful in his operations, many of which have presented unusual complications and difficulties.

The results of the treatment of uterine cancer have been especially gratifying.

The mode of reporting cases in detail imparts to the work a practical value totally unattainable by the statistical method. The work is copiously illustrated by plates, many of which are from photographs, and its mechanical execution reflects credit upon the government printing office from which it emanates. H.

The Physiology and Psychology of the Brain. By HORATIO R. BIGELOW, ESQ., Boston, Mass.

A small pamphlet containing nothing particularly new, being a feeble effort to argue God out of existence. As a specimen of the style of argument designed to accomplish this end, we quote :

"The present tendency is to look upon mind as the force generated by the chemical re-actions in the vesicular neurine of the cortical and hemispherical cells of the brain." * * * *

"The will is a product of the mind, and hence, as a dependent force, the paradox of free will becomes apparent." * * * *

"And just as the hand by constant use may become palsied, so the mind by the constant inspection of this one idea, breeds unhealthy chemical action in the cells of the brain, and originates," etc., etc. That is to say, the mind is a force generated by chemical action—the mind breeds unhealthy chemical action, *i. e.*, the mind is generated by its results, or the causes of mind result from mind.

The publication of a little more of this sort of logic, will certainly tend to induce the conviction that some minds are generated by chemical action, and that of a very unhealthy sort. H.

Body and Mind. An Inquiry into their Connection and Mutual Influence, specially in reference to Mental Disorders: an Enlarged and Revised Edition, to which are added Psychological Essays. By HENRY MAUDSLEY, M.D., Fellow of the Royal College of Physicians; Professor of Medical Jurisprudence in University College, London, etc. London: McMillan & Co. 1873.

So rich a literary and intellectual treat as is supplied by the perusal of this little book, rarely falls to the lot of the medical student, and we shall mark the day devoted to it with a white stone.

The author is a master of physiology, and presents that of the central nervous system in a manner as attractive as instructive. He is certainly entitled to the thanks of all investigators in the higher regions of physiology, for having formulated in language some of the ultimate facts in nervous physiology, which, while recognized as truths by some, are, to the many, vague, indistinct, and altogether "without form and void."

The author pushes his investigations into the nature of mental functions and processes from a physiological stand-point, rejecting altogether metaphysical data as bases for conclusions, and we cannot help feeling that he is a little too exclusive in this rejection of all *a priori* conceptions.

There appears to be no essential difference between the theory of the creation of what is termed mind as an entity, and of a nerve-cell which is claimed as the agent of its production.

Whether the investigator proceed upon the theory of progressive development of organisms, or of specific creation, he must, sooner or later, reach a limit beyond which his inductions will not carry him, here he must stop, even the theory of spontaneous generation will not aid him; "*ex nihilo nihil fit*," in the natural order. While we do not presume to insinuate that so great a mind as that of Maudsley could be tainted with atheism, such is, nevertheless, the necessary result of these development theories, if they are pushed to their logical consequences, of which he very prudently stops short.

The critical essays in the latter part of the book are extremely fascinating. The character of Hamlet is analyzed in a masterly manner, the treatment of the subject proclaiming its author to be

as much at home in the realm of belles-lettres, as in those of physiology and psychology. H.

Skin Diseases, Their Description, Pathology, Diagnosis and Treatment. By TILBURY FOX, M.D., London, Fellow of the Royal College of Physicians of London; Physician to the Department for Skin Diseases in University College Hospital; Fellow of University College. Second American, from the Third London Edition; Rewritten and Enlarged, with a Cutaneous Pharmacopœia, a Glossarial Index, and Sixty-seven Additional Illustrations. New York: William Wood & Co. 1873.

"To be a successful dermatologist, it is necessary to be a well informed physician," says the author of the above named treatise, and its careful examination will convince the student that it is the work of one who can lay claim to both these distinctions. The earlier chapters upon the method of study, upon the anatomy and anatomical consideration of the skin and its appendages, upon its general pathology, etiology and therapeutics, are especially well systematized and lucid. The chapter on classification presents sound, new and meritorious ideas, and the glossarial index will be found a great convenience to the busy practitioner. The descriptions of the less common forms of disease are numerous and from reliable sources. The work is profusely illustrated with woodcuts—many of which are new. H.

The Cerebral Convolutions of Man; represented according to Original Observations, especially upon the Development in the Fœtus. Intended for the use of Physicians. By ALEXANDER ECKER, Professor of Anatomy and Comparative Anatomy, in the University of Freiburg, Baden. Translated by ROBERT T. EDES, M.D. New York: D. Appleton & Co. 1873.

A laborious and faithful analysis of the lobes, convolutions, and fissures of the brain, aptly illustrated with diagrams in outline, which cannot fail to be instructive to the diligent reader. The author has been a faithful student of the literature of his subject, which being for the most part in French and German, is inaccessible to the majority of American readers. The translator has done his work with remarkable fidelity, and has demonstrated his knowledge, not only of German, but, what is still more rare, of accurate English. H.

Epidemic or Malignant Cholera. By ALFRED STILLE, M.D., Professor of Theory and Practice of Medicine in the University of Pennsylvania. Reprinted from the Philadelphia Medical Times. Philadelphia: J. B. Lippincott & Co. 1873.

A brief summary of the history, etiology, pathology and treatment of this formidable malady, from the pen of one of the most accomplished members of our profession. The reader will be disposed to find fault with the brevity of the pamphlet, and regret that the doctor has not entered more fully into the details of treatment. It is satisfactory to know that he repudiates the theory of the common origin of cholera, and claims for it a specific origin and an invariable source.

Those who are doubtful upon this head, should dispel their doubts by the careful perusal of this little monograph, in which their time will be by no means wasted. H.

Mineral Springs of North America—How to Reach and How to Use Them. By J. J. MOORMAN, M.D., Physician to the White Sulphur Springs; Prof. of Medical Jurisprudence and Hygiene in Washington University, Baltimore, etc., etc. Philadelphia: J. B. Lippincott & Co. 1873.

The Mineral Springs of the United States and Canada, with Analyses and Notes on the Prominent Spas of Europe, and a List of Sea-Side Resorts. By GEO. E. WALTON, M.D., Lecturer on Materia Medica in the Miami Medical College, Cincinnati; Committee of the Medical Association of the State of Ohio, on "Therapeutics of Mineral Springs." New York: D. Appleton & Company, 549 & 551 Broadway. 1873.

Editorial.

To Correspondents.

Induced by an earnest desire to comply with the wishes and advance the interests of correspondents, we have occasionally admitted to the pages of the JOURNAL communications which our judgment, unbiased, would have induced us to reject. To protect our pages from the introduction of any matter not in the highest degree worthy, and to secure to our readers that to which they are justly entitled, *i. e.*, original matter from reliable sources, we shall

hereafter publish no article from an unknown source unless it be accompanied by a reference to some resident practitioner in good standing who will vouch for the good faith and respectability of the writer.

The JOURNAL will not be degraded into an advertising medium for individual pretension, but a permanent record of professional progress.

"The Abuse of Medical Charities."

The *N. Y. Medical Record* (Sept. 1, 1873,) contains an admirable editorial upon "The Abuse of Medical Charities," which concludes with the following: "When a profession can claim to treat gratuitously three hundred thousand paupers annually in New York City, not counting those in hospital, and knows at the same time that one out of every ten of the young medical men in that city can barely earn a living, we think it is about time to look into the matter, to consider whether the occasion is not opportune for proving the force of the proverb, 'Charity begins at home.'" We hope that every reader of the JOURNAL will read the entire article and make the application to his own locality, to which it is doubtless quite as appropriate as to that for which it was written. For the little consideration which the medical profession enjoys throughout this country, it is indebted to some of its own members, who, avaricious of practice and notoriety, have sought to acquire them by cheapening their own services, and thus taught society to fix a very low valuation upon that which is eagerly offered without fee or reward.

The world's standard of value is cost; that which costs little is, in the popular estimate, worth as little. At the present time the pay of the average physician is actually lower than that of the average mechanic, relatively very far below; and why? because the public knows it must pay for the services of the mechanic, whereas those of the physician can be obtained for nothing, or, at the most, for a promise to pay which is never meant to be redeemed, and of which the average physician has not the moral courage to enforce the fulfillment.

This cheapening of their profession works a double injury and perpetuates itself; first, by oppressing the young and obscure, whose services are esteemed valueless when the elders hold their own so

cheap; and secondly, by thus discouraging the entrance into the profession of those who would maintain its honor and advance its interests. Thus the professional standard is actually approximated to that which it is made to appear, by some pseudo-philanthropists in its ranks.

The medical profession appears of late to have abandoned its *esprit de corps*, and its members to have concentrated and individualized their interests; instead of elevating the honor, dignity and importance of their profession and therewith their own as of integral parts of it, too many seem to be striving to rise alone in solitary grandeur upon its ruins.

There is one method of elevating the dignity of the profession, and of increasing the value of its services, which is within the reach of every physician; a method entirely within the comprehension of the public, and which it will most thoroughly appreciate, which if adopted and maintained consistently and persistently, will in one single year advance our profession in the public respect to a higher degree than can be accomplished in ten, by any other means. The method is this. Be strictly conscientious in rendering the best services within your capacity to your patients, and equally conscientious to yourselves in exacting a full equivalent for them, to be paid when the services are rendered; not at the end of the month, quarter, or year, but on the spot. In other words, treat your patient as he treats you. Let us ask no protection for our profession, no privileges, but simply the privilege to protect ourselves.

When you are asked for charity you will then have the wherewithal to do charity, you will be able to give out of that which you have received. In plain English, adopt the cash system as the rule of compensation for professional services, and we can assure those who will try it for one year, that they will never abandon it.

Experto crede. H.

The *New York Medical Journal*, for September, 1873, announces that "Dr. Walter Hay, of Chicago, has been appointed Adjunct Professor of Principles and Practice of Medicine in the Western Medical Institution." This is news indeed to the one most interested. We had the honor to be appointed to that position in Rush Medical College, but "the Western Medical Institu-

tion" is something which we know not of. Will the *New York Medical Journal* please relieve us of the unmerited honor and oblige

H.

Foot.

Treatment of Traumatic Neuralgia. By S. WEIR MITCHELL, Physician to the Philadelphia Orthopædic Hospital and Infirmary for Diseases of the Nervous System. ("Injuries of Nerves, and their Consequences,") 8vo., p. 337. London: Trubner & Co. 1872.

In the treatment of traumatic neuralgia the author says:

"The pains of traumatic neuralgia are so terrible that we are usually driven at once to the use of narcotic hypodermic injections, without which it would often be impossible to relieve such cases. In neuralgia, from what we call, for want of a better term, irritation of the nerves, there is reason to believe that some of the opiates, in the form of hypodermic injection, may prove more or less curative in their action; but where, as in most traumatic neuralgias, there is manifest organic alteration of the nerve, such agents are chiefly of service, because they relieve pain, and thus enable us to bridge over, so to speak, the many months of torture which are needed to bring the nerve back to health again, or to afford time for electrical or other treatment. * * * At the present time, this mode of using narcotics has grown into common use, but even yet it is scarcely estimated at its full value. In the wards for nerves and wounds in the U. S. A. Hospital, it is almost the only plan of treating severe neuralgic pain; so that twice or thrice a day the resident surgeons pass around these wards with their narcotics and hypodermic syringes, seeing, as a physician observed to me, anguished and troubled faces before them, and leaving behind them comfort, and even smiles.

"During one year at least forty thousand doses of various narcotics were administered without an accident, and in certain single cases upwards of five hundred hypodermic injections were used; so that, if there were no other evidence of the innocence of this mode of medication, our own experience would have been amply competent to settle the question. I have had large opportunity of studying, in traumatic neuralgia, the comparative merits of narcotics used by the mouth or under the skin, and I have no hesitation in stating my opinion, that the latter method is not only the more effective, but also the less harmful, constitutionally. For the easing of neuro-traumatic pain we tried, in turn, the whole range of medicines known as narcotics, such as conia,

hyoscyamus, daturia, atropia, and morphia. None of them, save the last, seemed, when singly used, to be of the slightest value; and, one by one, they were laid aside, until in the vast mass of cases, the salts of morphia alone were employed. A careful series of examinations showed, very distinctly, the trifling influence of atropia upon this form of pain. Several cases of intense neuralgia from wounds were treated with injections of sulphate of morphia under the skin in rising doses. When we learned the amount needed to give entire ease, we used in its place next day a full dose of atropia, our largest injection having been one-fifteenth of a grain. The most absolute failures attended these efforts; so that without denying to this latter drug the power ascribed to it of relieving certain neuralgias, I am sure that it is in the traumatic species simply useless. The morphia salts, on the other hand, are invaluable. For hypodermic use, I usually employ at first one-fourth of a grain of the sulphate, but I have given as much as a grain and a half twice a day. When continuously used, it is curious that its hypnotic manifestations lessen, while its power to abolish pain continues; so that the patient who receives a half grain or more of morphia may become presently free from pain, and yet walk about with little or no desire to sleep. The ability to lessen pain is not, therefore, of necessity, connected with the sleep-compelling potency. Where, however, the latter is inconveniently felt, and we desire the former only, it is possible to attain the end in view by using with the morphia a certain share of atropia. Thus, if we inject half a grain of sulphate of morphia, and with it the thirtieth of a grain of sulphate of atropia, the anæsthetic force of the morphia will remain unaltered, but the tendency to sleep will be greatly diminished. The views here set forth were reached after long and careful experiments on large numbers of men, and seem to justify the practice of using atropia and morphia together. As regards the place of injection, I agree with most observers that it is generally of little moment, the effect being the same whether it be thrown into the affected limb or into a remote part."—*Half-Yearly Abstract, Med. Sciences, 1873.*

Mercury and Iodide of Potassium in Syphilis.

Dr. Willard Parker of New York, in a recent clinical lecture reported in the *Medical Record*, sets forth the following views:

"I am aware that I differ with many of my brethren in the treatment of syphilis, but I believe that the poison of syphilis can only be removed from the system in almost all cases by the judicious use of mercury. This mercury is to be used wisely and in moderate doses, so as not to impair the vigor and health of the system. Very often it is important to make use of some tonic at the same time, such as quinine or the preparations of bark. These have been my convictions for a great many years, and I give them as

the result of my own practical observation, and have never seen any reason to vary the conviction that iodide of potassium, alone, cannot overcome the syphilitic poison in the system. The iodide of potassium, however, is a very valuable remedy in the treatment of syphilis, but it comes in after we have accomplished our purpose with mercury, in order to remove any deleterious effects of the mercury which may be left in the system. Here its value cannot be over-estimated. The powerful effect which the iodide of potassium has upon the system, especially where mercury has been employed pretty freely, is sometimes seen in the profuse pyalism which it produces, and if the syphilis receives any benefit from the administration of iodide of potassium, I believe it is in those cases which have been heretofore treated with mercury, and the iodide arouses the mercury to new action. You can remove mercury from the system by the use of iodide of potassium, but you can never remove syphilis by using it. At the same time that we use iodide of potassium in order to get good results in the system, I almost always employ the iodide of iron, as you see in this case. The point is, as has been stated, to bring the system up to par. The usual formula which I employ consists of six drachms of the iodide of potassium, one ounce of syrup iodide of iron, and make a six or eight ounce mixture. * * * The plan which I adopt and recommend in the treatment of syphilis is as follows: Take a case of genuine Hunterian chancre. I commence with the administration of iodide of mercury in one-half grain doses twice in twenty-four hours, combined with something, perhaps hyoscynamus or lactucarium, to prevent irritation of the mucous membrane of the intestinal canal. Continue this, in connection with a true diet, consisting of simple plain material and such as will produce healthy blood, embracing breadstuffs, eggs, milk, and meat twice a day, and cutting off entirely tobacco and all alcoholic drinks; continue the doses until the feeling of hardness about the chancre is all gone. Then stop the remedy, and watch the patient. If the disease begins to come out in the system, manifesting itself by glandular enlargements, diseases of the skin, affections of the fauces, or any one of these evidences, which shows that the poison is still in the system, resume the mercury as before and continue it until the disease has again passed away. It will be necessary to watch these patients for a long time, at least for months, and perhaps for a couple of years or more."

Capital Punishment.

We will doubtless all unite in the opinion that certainty of execution is the chief value of all penalty. This *uncertainty*, or rather almost absolute certainty that the extreme penalty will not be inflicted, is an encouragement to the murderer that his crime will somehow go unpunished. These questions of criminal law, how-

ever, are not so much a matter of professional inquiry or interest as that other question—how shall the extreme penalty of the law be best fulfilled? The question of humanity should never interfere with the power and influence of law; but if the control of law and the sanctity of humanity can be made to accord, all will certainly say 'amen!' If it be well to make death the penalty of certain crimes, the question comes to medical men, how shall this extreme punishment be inflicted so as to meet the requirements of justice, and yet avoid unnecessary cruelty in its execution. In various countries, death has been secured by hanging—as in this country—by the garrote, by the guillotine, by shooting. Some experiments by Prof. Brown-Sequard would seem to indicate that death by the guillotine is perhaps one of the most cruel of all known modes. Of course a headless man does not come back to tell us how long or how extremely he suffered; but we are led to infer that the cruelty of this punishment is beyond the general conception. Death by hanging is, to say the least of it, coarse and brutal. Its influence on the public mind cannot be elevating or conservative of morals of itself. So we come back to repeat, *extreme* punishment, as by death, is only of value, so far as penalty follows closely on crime. Now, if death shall continue to be a proper penalty for murder, the question for us, as medical men, to consider, is, how shall death be inflicted? Shall we shoot the culprit, or hang him like a dog, or cut off his head? The displays to morbid appetite in first-class hangings are too notorious to require comment. We respectfully suggest, then, that our professional influence should be exerted in favor of a more rational and decent death penalty. Let the criminal be required to die by chloroform, for example, or the inhalation of carbonic acid gas, quietly, in his cell, surrounded, if you please, by his medical and spiritual advisers. Death by chloroform is sufficiently certain; should not be a matter of public and morbid exhibition; is not cruel; all the requirements of the law are complied with. We demand, then, these two points—certainty of execution and humanity of the manner.—*Cincinnati Lancet and Observer*.

Imposition on Life Insurance.—*A New Department of Medical Journalism. An Interview between a Medical Journal Canvasser and a Medical Officer of a Life Ins. Co.*

Ques. "Doctor, I have called to present you with a copy of our *Medical Journal*, and call your attention to the new branch of medical science we have lately introduced. Don't you think the subject of Life Insurance a very important one to bring before the medical profession?" Ans. "Certainly." Ques. "Well, you see, we call it the 'Medical Department of Life Insurance.' We solicit from the medical officers of all the Life Insurance Companies articles bearing upon this subject, and would like to have you

furnish us one for our next number. We wish to make this department instructive, especially for the benefit of medical examiners throughout the country, and it will be a very cheap way for Life Insurance Companies to advertise and furnish such kind of information. Several of the larger companies have already agreed to take several hundred copies to send to their examiners, as we can furnish it to them at a much reduced cost from the regular subscription price." Ans. "Ah, I see; then the whole gist of the matter is, that your publishers propose, by extracting what little phosphoric elements may be squeezed from the brains of the companies' medical officers, without compensation, to illuminate their rustic brethren in the same line, and thus create a demand for new subscribers. In this way you may 'hit two birds with one stone.' Well, this may be all very well for increasing publishers' incomes, but don't you think it is a little like 'crowding the mourners'?"

"Now, I pay the full subscription price for your Journal, and you propose to add to your income by *peddling* to companies your Journal at half cost, at my and my medical brethren's expense. You wish to get valuable articles without cost, then induce companies to subscribe for a large number of copies at reduced prices, to circulate among their medical examiners, to induce these throughout the country to become subscribers, to make your Journal a pecuniary success, and also advertise your other published works.

"It really seems to me that the medical profession are already pretty well taxed, physically and mentally, in the various walks of this life. With gratuitous hospital and private services rendered the poor and needy, uncollectable bills, with income barely adequate for supplying themselves with the necessaries of life, to say nothing of numerous ephemeral medical journals, now to be called upon to contribute their 'mite' to swell the coffers of large publishing houses, by voluntary contributions, is asking a 'leettle too much.' I have always admired that trite old saying of Scripture, 'The laborer is worthy of his hire,' and in this age of 'strikes,' think the medical profession should join in refusing to work any longer for cormorant capital, without adequate compensation. I hope to see the day when medical journalism will be placed upon the proper business relation that daily journals are,—that its corps of contributors shall be compensated for their time and labor. Then we would see articles having true merit, full of instruction, and from the most learned in the profession, instead of the *advertising squibs* which now fill them. Don't you think the National Medical Association ought to interdict, in the code of ethics, publishing in medical journals articles with their authors' names and titles as specialists? If a medical gentleman has true merit in any special branch, his peers will soon find it out, without resorting to such doubtful means to catch the public eye."—*The Sanitarium*.

Life Insurance.

I am much gratified to see that you have added the medical side of life insurance to the many other interesting and practical features of your journal. The articles presented are all of much practical value, and just this kind of information is sadly needed by the majority of medical examiners. Permit me, as an examiner of some experience, to call attention to some points connected with the business, which I think deserve attention.

The article in the *Record* for May 15, entitled "On the Relation of the Medical Examiner to the Business of Life Insurance," by a Secretary, is very just and true, and certainly demands no more of examiners than a proper personal independence and appreciation of professional obligation would at all times prompt the *right-minded* to perform. But just here is the trouble, and the secretary evidently understands it, for he says it is this very want of personal independence which is far more dreaded by underwriters than any mere want of professional capacity. Unfortunately the two combined are not unfrequently met with, and I have seen and known of more than a few instances of such an utter want of both in the examiner as to make him the *subservient tool* of the agent. It is the agent's business to solicit risks, and it is his pecuniary interest to have those risks approved at the home office; so he is never backward in using an ass of a doctor if he can. And not unfrequently does it occur that the agent purposely avoids giving business to a man he knows to be competent and independent, preferring a more *compliant* examiner.

I have been cognizant of this state of things for so long a time, and have seen but little, and very imperfect, effort made for its correction, that I have begun to think the companies were indifferent about it—supposed they were making fortunes rapidly under this kind of procedure, and did not care to make any additional effort to correct abuses. If a remedy for it should be sought, I would suggest that one step in the right direction has been taken in securing the co-operation of your journal; another, and most prompt and decided check on these abuses, would be the securing of *competent and reliable medical men* as inspectors of agencies, paying such salaries as would command competent men, who would give the business their entire time. A competent medical man would, by the very nature of his professional training, and sharpened powers of observation, be capable of going into any agent's district, making the acquaintance of the physicians acting as examiners, and *others not so acting*, and render a far more correct opinion, *cæteris paribus*, than a non-professional inspector.

Permit me, in closing, to suggest that according to my observation here in the South, the tables of average weight to height are at least ten pounds higher than a correct estimate. MED. EXAM.

—*Medical Record.*

Reports of Societies.

Brainard Medical Society.

The Brainard Medical Society met in the Christian Chapel, in Winamac, Ind., June 12, 1873.

Members present: F. B. Thomas, James Thomas, A. R. Thompson, W. T. Cleland, G. W. Nafe, Le Roy Rogers, Wm. Kelsey, D. F. Moss, H. E. Pattison, W. H. Thompson, G. W. Thompson, I. B. Washburn, and J. W. C. Eaton.

The President, Dr. Glazebrook, being absent, the Secretary called the Society to order, and introduced the President elect, Dr. I. B. Washburn, of Logansport.

Before taking the Chair, he read his inaugural address, which elicited the closest attention. A copy of it was requested for publication in the Winamac and Logansport papers.

On motion of Dr. Kelsey, T. J. Loring, medical student, was elected a member of the Society.

Dr. Eaton, of Pulaski, read a paper on the Pathology of Typhoid Fever.

In his paper, Dr. Eaton objected to the term typhoid, but said the typhoid state would be preferable. He said the products of tissue metamorphosis are chiefly eliminated by the kidneys. As long as the kidneys are equal to the increased work, the blood is properly depurated and the typhoid state warded off. But if, from the large amount of effete matter, or from any other cause, the kidneys be not equal to the task, the blood becomes poisoned, and either convulsions or the typhoid state supervene.

The paper was discussed at length by Drs. Cleland, Kelsey, Rogers, Pattison, Nafe, and Washburn.

The Society adjourned to meet in Star City, August 7, 1873.

J. W. C. EATON, Sec'y.

Cass County (Indiana) Medical Society.

A number of the physicians of Cass county, Indiana, met in the Court House, in Logansport, and organized a County Medical Society, by adopting a constitution and electing the following members officers for the ensuing year:

President—J. A. Adrain, Onward.

Vice-President—W. H. Bell, Logansport.

Secretary—J. H. Goodell, Walton.

Treasurer—J. M. Justice, Logansport.

Censors—Asa Coleman, Logansport; James Thomas, Royal Center; I. B. Washburn, Logansport.

The Society adjourned to meet in November.

J. H. GOODELL, Sec'y.

